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LECTURES
ON
COMPARATIVE ANATOMY
AND
ANIMAL PHYSIOLOGY,

DELIVERED BY

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LECTURE XVI.

ON THE OSTEOLOGY OF SAURIAN REPTILES.

THE saurian reptiles have a more highly developed and more complete skeleton than the serpents, as, in addition to the parts of the simple skeleton of the *aphidia*, we have here a complex sternum and scapular apparatus, a pelvic arch, together with atlantal and sacral extremities to raise the serpent's trunk above the ground. This is the group of reptiles which comprehends all the crocodile and the lizard tribes, which have distinct organs for progressive motion, and moveable ribs. In the imperfect development of the extremities, and in the moveable condition of the bones of the head in many of the lizards, we perceive a gradual transition from the worm-like form of serpents to the massive forms of the loricated or crocodilian reptiles. By the increased development of all the processes of the vertebral column, we perceive the preparations for more solidity in the articulations and more limited motions in that portion of the frame-work. The swimming motions of aquatic animals, performed by their vertebral column, and the creeping motions of serpents, performed by the ribs, are now to be changed for motions of walking, running, climbing, flying, performed by members at a greater distance from the centre or axis of the skeleton. The inferior grade of development is still indicated by the coarse fibrous texture of the bones in all the larger species of sauria, recent and fossil; and the same inferiority is seen in the large bones of the cetaceous mammalia—from the great proportion of animal matter compared with the earthy materials of the bones. And in most of the long bones of the extremities of reptiles, the cancellated structure occupies the whole extent, leaving no continuous cavity in the middle, as we find in mammalia and birds. In this they present an analogy with the embryo cartilaginous condition of those bones in man, when there are no internal canals even in the long bones.

In the vertebral column of the monitors and other lizards, we observe that the ball-and-socket joint continues, as in the serpents, as in the crocodiles, as in the chelonians. A deep concave cup is seen on the body of the vertebra at its anterior part; the posterior part presents a transversely elongated ball. This mode of articulation passes down through the sacral and through the coccygeal vertebræ. In serpents we have seen that there can be no sacrum, for the same reasons that there can be no sacrum in fishes or in cetaceous animals, but it is otherwise in the animals we are now come to. This fixed condition, given by the attachment of posterior extremities, through the medium of a pelvic arch, produces a sacrum, but there are no legs to fix by their attachment any part of the vertebral column of serpents. Why, therefore, should any two of their vertebræ be more limited in their motion, and thus from a sacrum, than any others? All the vertebræ from infancy admit of the freest motion, and there is no reason why any two should anchylose or exhibit a more fixed condition than others, therefore there can be no sacrum in the serpents; but in the saurian reptiles there are legs, whether employed to swim through the water, or to support the

trunk on the dry land. In former conditions of our planet, among the saurian reptiles, there have been species of gigantic size, that have swum through the estuaries and seas in our own latitudes, and have left their remains in thousands in the old secondary lias formations of this country—reptiles whose enormous bones put to dwarfish insignificance the most stupendous of the gavials ever witnessed on the banks of the Ganges, or the most frightful crocodiles that ever infested the banks of the Nile. Those have gambolled about in our own latitudes, with posterior extremities formed like the arms of a dolphin, but where there appears to have been no fixed sacrum for their attachment.

We observe, however, that the existing crocodiles, the gavials, and other animals of that tribe, which abound chiefly on the banks of the great rivers and estuaries, have the posterior extremities attached by means of the pelvic arch to the enlarged transverse processes of at least two vertebræ, and this connexion of the posterior extremities in most of the land sauria with the vertebral column produces a sacrum. We find that to be the case, for instance, in those lizards, as well as in the crocodiles, and other loricated saurian reptiles. That sacrum still consists, however, of but few vertebræ—generally two—two vertebræ that are almost immoveably fixed. In this specimen of the great monitor of Java, you observe, again, a sacrum consisting of two vertebræ, with their transverse processes much enlarged, and presenting a broad articular surface at their terminations. There is a gradation then from the frog, which we have seen has but one vertebra forming the sacrum, through the saurian reptiles, where there are generally two, to the most complex form of that bone in birds.

In the coccygeal region we observe the great development of inferior spinous processes in most of these animals, and that those elements are not united by anchylosis to the bodies of the vertebræ. So that taking these vertebræ in their most common form, we have in the saurian reptiles distinctly developed the transverse processes for muscles, the superior laminæ for the spinal marrow, and the inferior laminæ for the continuation of the aorta, and from that the spinous processes are continued downward. Extending along the coccygeal vertebræ of these monitors and iguanas, we see both the upper and inferior spinous processes diminishing; the transverse processes we find diminishing also until there are left at the extremity only long cylindrical bodies, forming a lengthened tail, flexible like a whip. This is important to many of those lizards, for like the iguana and others, we find them twining this whip-like extremity round the branches, to give them security in their movements upon the trees. In the iguana of America we observe that the tail is like a long cylindrical whip, and, as is the case with very many of these saurian reptiles, they are accustomed to reside much upon trees. There they encounter the numerous inhabitants of trees, squirrels, birds, bats, monkies, and other warm-blooded animals. These more highly organised residents of the trees find their antagonists among the cold-blooded animals of the present class, the numerous serpents which lurk among the foliage, and the active lizards, with their long flexible fingers, their sharp curved claws, and their prehensile tail, which are able to run up the perpendicular trunks and branches, as if they were moving on a horizontal plane.

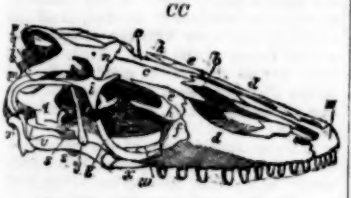
The articular processes of the vertebræ are generally longer than in the ophidian reptiles; they are more narrow, extend directly backwards, and allow of a more limited motion between them at each articulation. The bodies of the vertebræ are also more lengthened, which limits the flexibility of the whole column, compared with that of serpents. The most anomalous forms of the bodies of the vertebræ met with in the saurian reptiles are those of the extinct *ichthyosaurus* and the gecko, where they terminate anteriorly and posteriorly in funnel-shaped cavities, as in fishes and aquatic batrachia. It is interesting to observe, that in the larger forms of this order not only do the elements of the cranial vertebræ remain permanently detached, but also the elements of succeeding vertebræ of the trunk; and this we see both in the recent and extinct species; the laminæ are connected only by suture with the body of the vertebræ. The inferior spinous processes remain detached and moveable, and are attached nearly at the posterior end of the body of each vertebra, so as to appear as if placed below the intervertebral spaces. In fishes these inferior spinous processes are formed by the descent and junction of the transverse processes, and are continuous with the body of each vertebra. In the crocodiles the detached elements are very conspicuous in the cervical vertebræ, and the broad, detached, transverse processes in that region form so many ribs descending from the sides of the cervical vertebræ, and thereby limit the lateral motion of the neck. The anterior cervical vertebræ in the *sauria*, however, are generally free from ribs, as in higher classes, so as to have greater mobility in that region of the column, and the number of these free cer-

vical vertebræ is about the same as in the neck of mammalia. There are false ribs anterior to those which reach the sternum, as well as behind them, as we see also in birds and some mammalia.

Although we observe remarkable differences in the skeletons of this very extensive order of reptiles, we can for the most part trace in these varieties mere gradations of the same general plan of formation. From the lowest tribes upwards there is a gradual consolidation and union, not merely of the elements of the several bones with each other, but the aggregate bones formed by these united elements become more and more firmly articulated with each other, so as to lead us insensibly to the more solid skeletons of mammalia and birds.

This is nowhere more obvious and remarkable than in the bones of the head. In this skull of the lizard of the Nile (*lacerta Nilotica*, Cuv.) (*CC*) you at once

CC



perceive the detached and moveable and serpent-like condition of all the bones of the face, and nearly of the whole head. The occipital bone has its transversely elongated condyle composed chiefly of the basilar portion, which is sometimes in these inferior *sauria* anchylosed to the two condyloid pieces (*CC*, *q*, *q*). The squamous and the mastoid elements (*CC*, *l*, *m*) of the temporal bones are here remarkably long, slender, and curved, extending backwards and downwards to the strong, short, tympanic element (*CC*, *r*) to which the lower jaw is attached on each side. The two parietal bones (*CC*, *n*) are here, as in serpents, anchylosed into a single piece, and are remarkably expanded at their anterior margin to give a broad surface for the articulation of the two elongated middle frontal bones (*CC*, *c c*). The posterior frontals (*CC*, *i*) are very small, and form the posterior and upper boundary of the large bird-like orbits. The squamous portion (*CC*, *p*) of the temporal bone is here the largest and strongest element of that bone, extending forwards to the sphenoid (*CC*, *s*), and backwards to the slender squamous (*CC*, *l*) and mastoid (*CC*, *m*) portions. The lachrymal bones (*CC*, *f*) extend less over the face than in these serpents, and the two divisions of the upper jaw (*CC*, *d*) are larger and more fixed than in serpents. The anterior frontals (*CC*, *e*, *e*) have a more lengthened form, and extend more downwards over the face, than in the former order. The two narrow lengthened nasal bones (*CC*, *b*) are anchylosed into a single piece, and the same is the case with the two intermaxillary bones (*CC*, *a*), which expand laterally, and form the whole anterior boundary of the muzzle, containing numerous small teeth. The inferior turbinated bones, here widely exposed between the upper jaw-bones, in consequence of the small size of the nasal bones, present a great surface for the first pair, or olfactory nerves. The palatine bones are less moveable than in serpents, and destitute of teeth, so that the distribution of the teeth is becoming more limited as we ascend to higher forms of vertebrata. The pterygoid bones (*C, C v*) are more strong, and extend backwards, as in serpents, to the articulation of the tympanic bone with the lower jaw. The lower jaw is divided at the symphysis, and consists on each side of six pieces, of which the anterior or dental portion is the largest, and the teeth are as loosely attached by their expanded base to the alveolar flat surface of the jaw as in serpents.

The general physical characters of the teeth are interesting, particularly in many of the saurian animals I am now considering. Their osseous substance and their enamel are softer than those of mammalia. The enamelled teeth of carnivorous quadrupeds have the greatest density, both in their thick enamelled surface, and in the osseous substance of their interior, so that if you take the sharpest steel instrument, and draw it over the surface of the osseous part of the teeth beneath the gum, you will make a very slight groove, like a mere scratch, upon the osseous substance; but when you come to the enamel, you might as well press the instrument on a diamond; it slides over the surface, leaving a dark metallic mark of the hard steel upon the enamel, but makes no impression or groove. When you try this upon some teeth that to the naked eye appear as if they were all surrounded with enamel, as the canine teeth of the hippopotamus, you will find the knife scratch the teeth on the concave inner side, where there is no enamel, but that when you come to the outer convex part, where the enamel is placed, the instrument slides over and leaves no mark. The importance of this to geologists is well understood by those who have directed their attention to it; but what interests us now is, that both the enamel and the osseous sub-

stance of the teeth diminish in density as you go down the scale. This is a curious fact, for when you try the crocodile's teeth, which you know are covered with enamel, you find their enamelled surface actually yield like the internal osseous substance of the teeth of the quadrupeds; yet they exhibit considerable density compared with the coarse bones of the skeleton.

The teeth of the crocodiles are hollow cones, striated longitudinally, and open at their fixed base. The new teeth are seen in the young crocodiles beneath and within the old teeth, pushing them out from their alveoli to occupy their place. From the glistening exterior and compact appearance of these teeth, you would not expect them to yield so readily to the knife. This is the case also with the teeth of serpents. There is but little, comparatively, of the enamel disposed upon the teeth of serpents. In amphibious animals and in fishes, seldom do we find a trace of enamel; and the osseous texture composing generally the whole of the teeth of fishes, is remarkable for its softness. Were you not aware of the circumstance, that there are classes of animals in which the teeth are chiefly composed of osseous substance, it would perplex you to see the osseous margin of the jaws presenting a serrated appearance. You would be perplexed how to account for the bones of the jaws presenting that unusual appearance. But we frequently find in fishes these osseous teeth anchylosed to the substance of the jaws which support them, so as to leave no distinction between the teeth and the bone of the jaws. The application of these facts regarding the teeth, is sometimes of practical utility to the geologist. We observe, also, that teeth, even of the hardest texture originally, both in their osseous substance and enamel, become softer by remaining long exposed in the earth, without our perceiving any diminution of the glistening lustre of their surface. The anchylosis of the teeth to the jaw-bones in many fishes, gives them a fixed and solid character in the adult animal, which they had not in their younger growing condition.

There are numerous intermediate stages between the loose condition of the bones of the head we have seen in the lizards, and that remarkable fixed state of all the cephalic bones we see in these crocodiles, gavials, and alligators. This tendency to the fixed condition of the skeleton, we observe in many other parts besides the head; we see it in the neck, the scapular apparatus, the pelvis, the trunk. Their outward form shows how comparatively little motion is admitted of in all those parts, and that the motion of these animals in the living state must depend chiefly upon the lateral movements of their fish-like perpendicular tail. And, indeed, so it is in their living state, as we do not find them running like lizards up the trunks or along the branches of trees, but lurking among the trunks of trees and wood that have been washed down by the inundations of great rivers, and strewed over the banks of rivers and estuaries. They lurk in concealment for their prey, which they drag instantly into the water, and then commence all their active and vigorous movements. For those kind of movements, therefore, great solidity in all parts of the head is required, to hold their victim firmly, and to resist the violent efforts which it makes to escape. Great strength of the neck is required, and we find that given by the shortness of the neck, and by the extension of the very large transverse processes of the cervical vertebræ. The strength of the head is chiefly given by the junction, through the medium of sutures, of all the elements that enter into the composition of the bones of the head. Their cranial bones are all fixed, as in quadrupeds, and do not play upon each other as we see in the monitors, lizards, and many inferior animals.

The vertebral column of the crocodiles, the gavials, and the alligators,—that little family composing the loricated saurian reptiles,—those that are loricated, or have their coat of mail which you see covering the gavial I hold in my hand, composed of large solid scales placed across the back; these have the bodies of the vertebræ very long, and all their processes largely developed, and terminated at the posterior part of the body of each vertebra, with a round convex prominence, which is received into a cup-like cavity at the upper part of the next succeeding vertebra behind. It is remarkable to observe in the skeletons of these large saurian reptiles, the complete and permanent detachment of the elements of many of the vertebræ, and that particularly in the region of the neck, so that the cervical ribs of the crocodile are only detached transverse processes, and the superior laminæ of these cervical vertebræ are also joined permanently by suture, the spinous processes remaining thus permanently detached. In those reptiles the spinous processes are long and strong; the transverse processes are also broad and thick and strong. In the region of the neck you observe how broad the transverse processes are in the skeleton of the Indian gavial, and you observe the perforation for the vertebral artery, here, of great

length and breadth. But large as these vertebræ are, you observe how small the foramen is for the passage of the spinal marrow. In this skeleton of an Indian gavial, more than fifteen feet in length, the spinal canal will not admit my little-finger to pass through the foramen of a dorsal vertebra.

Notwithstanding the solidity in all parts of the head of the crocodiles, upon looking attentively at the surface of this skull of the crocodile of the Nile (*DD*), we

DD

see distinctly that the elements of the different bones have still remained detached, that they have met in the progress of their ossification, and that they have united only by sutures; so that we have, around the occipital foramen, the basilar bone, with its round prominent condyle detached from the two condyloid bones, and the two lateral extended condyloid bones detached from the superior very small median portion of the occipital bone,—the four elements existing in the human occipital bone. The parietal bones (*DD, m*), here remarkably small, have anchylosed above, as we see in most other reptiles. We saw that in serpents and in lizards. They form, therefore, a solid arch over the part of the nervous system enclosed within this remarkably small cranium. The temporal bone has its numerous parts permanently detached, and fixed together by sutures. One of those parts is interesting; it is the tympanic element or bone,—that bone which we saw already forming the os quadratum, moveable in fishes, moveable in the amphibia in serpents and in lizards; but here it is fixed, it is immoveably fixed, to the squamous portion and to the other bones of the head. It shoots down to a considerable extent, and terminates in a convex prominence or articular condyle, which is received into a corresponding articular groove in the lower jaw, as we see also in the *chelonia*. We observe that the malar bones are of great size and extent, forming the whole inferior and outer margin of the orbit. Passing inwards from the anterior termination of the malar bones, we come to two bones that are perforated within the orbit by the canal for the lachrymal duct; consequently we perceive these must be the lachrymal bones extending along the face; but within those two lachrymal bones towards the median plain, we come to two others, one on each side, that are also detached by sutures; these are the two anterior frontal bones; and, between those and behind them, the large middle frontal bones, anchylosed, and forming a large undivided median bone. The broad anterior and lateral part of the muzzle is composed in the crocodile chiefly of the large superior jaw-bones (*DD, b*); between those two bones, and extending throughout their whole length, we perceive the two long narrow nasal bones, covering a nasal cavity which extends to the occipital bone. The whole of that expanded round termination of the muzzle, is formed by two large intermaxillary bones, which have a wide nasal aperture between them. The sphenoid bone is remarkable here for its extent, and its development transversely. In fishes we saw that that bone was remarkable for its extent forwards, as it is also in the amphibia; but we are now rising in the scale, and making a nearer approach in the forms of bones to the condition of mammalia.

There are many curious analogies between these saurian reptiles, the most perfect of them, and the cetaceous or aquatic mammalia; so great, indeed, that one would seek for their origin, and the natural chain of developments, running through the extinct saurian reptiles to the cetaceous animals, to which they had many singular affinities. The large and long palatine bones support the median and posterior parts of the two superior maxillary bones. You will observe that the margin of the superior maxillary bones and the intermaxillary bones are penetrated by deep alveoli, that lodge teeth which have lengthened hollow fangs, and give all that fixed condition to the teeth of the crocodilida which we find in the jaws of the mammalia. Those teeth suffer often great stress from the vigorous resistance made by the larger quadrupeds, when struggling with the crocodiles under water. Crocodiles use the teeth not for mastication, but to hold the victim with, till it is suffocated, under water. Both must breathe atmospheric air, but the crocodile has greatly the advantage, for that mixed condition of the blood which circulates through its system, and the communication between the two sides of the heart, require not the quick succession of the acts of respiration which are necessary where the heart is completely divided, and where the whole of the blood must pass rapidly through the respiratory organs. That is not the case with any animals we have yet considered. Their

venous and arterial blood are partially mingled together, and pass on through the circulating system, often while the lungs are not exerted.

In the osteology of the head of the Indian gavial, we observe some slight differences, which give rise to remarkable distinctions in the outward forms of the head. Generally, the disposition and number of the bones of the skull are the same; the differences of form affect mostly the bones of the face. In the head of the Gangetic gavial, we observe that the upper jaw is composed throughout the greater part of its extent of the two upper jaw-bones alone, without the intervention of the nasal bones. The extremity of the muzzle, which is dilated and round, is composed entirely of the two intermaxillary bones; and we do not find a trace of the nasal bones, until we have passed upwards from the intermaxillaries, for more than half the extent towards the frontal bone. We come upwards to the extremities of the two nasal bones, tapering to fine points, and thrust narrowly downwards between the two superior jaw-bones.

Now, when you reflect upon this circumstance, you can deduce from it an illustration of a general principle, that where it is necessary to give great solidity and strength to a part composed of solid bones, the fewer sutures that are admitted the better. Were the nasal bones of the narrow-muzzled Indian gavial to shoot down along the whole length of the superior maxillaries, as you see in the crocodile, this very lengthened and narrow muzzle would be actually composed of slender strips of bone. There would be no less than three distinct sutures traversing the whole narrow face longitudinally above, whereas there is but one suture, by the two nasal bones being confined entirely to the upper enlarged part of the face, at the base of the long muzzle. The crocodiles are like the sharks among the fishes, with an endless supply of teeth, but in the crocodiles they are hollow cones which enclose each other, while in the sharks they are thin and solid-like lancets, and placed in contact behind each other.

There are scarcely any parts of the skeletons of the saurian reptiles more various in form than the scapular and sternal apparatus, which are so intimately connected with the various uses to which the arms are applied in the living state. These parts are more subdivided and complex in the inferior lizard tribes than in the quadruped forms of the larger *sauria*. The posterior portion, or dorsal part, of the scapular bones, is, in the lizards, generally very broad, thin, flexible, and semicartilaginous, as we already saw it in inferior classes. The osseous part of the scapular bone, which reaches to the glenoid cavity for the head of the humerus, is strong, thick, and broad, and is united only by cartilage to the very broad, thin, expanded, coracoid bone, which passes down to the middle piece of the sternum. The acromion process is also detached and lengthened. The clavicles are generally united as in birds, and very slender bones.

In the skeleton of the Nilotic crocodile (*EE*) you perceive that the whole of this apparatus has a more simple and more consolidated form. The two scapular bones (*EE*, *a*) are more lengthened and

EE

narrow, their posterior cartilaginous portion is smaller, and the coracoid bones have the same lengthened, narrow, and compressed form to their junction with the sternum. The middle portion of the sternum extends far forward upon the neck, beyond the other elements of that bone.

There are two pairs of false ribs before, and two pairs behind the true ribs in the crocodile (*EE*, *q*). The true ribs are attached securely by a double head to the notched extremities of the long transverse processes of the dorsal vertebræ. We have now come to animals where we have all the kinds of vertebræ met with in the most perfect of the mammalia; cranial, cervical, dorsal, lumbar, sacral, and coccygeal vertebræ. The true ribs approach to those of birds in having their ventral extremities attached by cartilage to solid ossified sternal ribs (*EE*, *q*) extending from the sides of the sternum.

On the anterior part of the abdomen of the crocodile we observe transverse ossified parts, extending forwards and inwards to meet on the median plane, like abdomi-

nal ribs; these thin transverse ossified pieces have an analogy in nature and position with the tendinous intersections of the *recti-abdominis* muscles. These recti-abdominis muscles of the human body, as you well know, are not continuous muscles from their upper to their lower extremities, but are divided by tendinous intersections, which tendinous parts in muscles we find frequently to become the seat of ossific depositions—to become ossified solid bones. We find that the tendons of muscles much exerted as here, are particularly liable to ossification, especially in animals where the ossification has arrived at its maximum of development. The tendons of the legs in the wading birds, and in many of the gallinaceous birds, we find commonly converted into solid bones. That is an illustration from another part of what we see here in the course of these powerful muscles of the crocodiles. Cartilaginous substances you might indeed expect to become frequently ossified in animals, because all bones were cartilaginous before they were converted into bones; so that it is nothing remarkable to find that if a quadruped shall require to exert great pressure upon the cartilaginous septum of the nose, it should have that part ossified. It ought to have it ossified, and it has it so. We are not then to look for something extraordinary in the nose of a hog because it has a bone at the end of the cartilaginous septum, as if it had some part there—an element of the skeleton—different to what exists in the human face.

These are cautions teaching us to seek for the common elements of structures, and to watch the operations of nature in varying the formation of parts before we immediately thrust in new elements of structure for a special purpose belonging to a particular animal. The difficulty that is created by that is not at first thought of. The difficulty that results, for instance, from assigning to the side of the head of fishes, opercular bones, as bones which do not exist in any form in man, is manifested by ascending higher and higher in the continuous series of vertebrated animals. What has become of the bones? If they be confined within the tympanic cavity or anchylosed to the angle of the lower jaw in man, they were not formed solely for the fishes, but still exist under another form in the human skeleton. So in every other case where we imagine that we have got elements that do not exist in other animals, but only in the particular species we are examining, as we see that throughout the whole of nature's organic productions she is constantly passing by imperceptible gradations, and never by sudden starts from one form to another, so you might at first imagine that the solid osseous shell which covers the whole abdominal surface of the tortoise and the turtle was something that did not exist in the human body. So, again, the large upper convex shield or carapax, so solid that it covers the whole animal above; we might imagine there was something there different from what exists in the human skeleton. These parts, however, we shall see presently are common to man with most other vertebrated animals.

The crocodiles require the muscular apparatus to be vigorous in the region of the abdomen, for the violent struggles which they have to sustain under water in obtaining their food; and they require to have a solid attachment for the muscles at the base of the enormous perpendicular tail, as well as for the extremities for progressive motion through the liquid element. Geoffroy Saint-Hilaire conceived, however, that the ribs which extend across the abdomen might be subservient to the same purpose to which the ribs at the anterior part of the trunk are chiefly applied; that they might be useful in the act of respiration; and in observing that the crocodiles, although timid and sluggish animals on land, display the most vigorous and lively muscular actions when they are in their natural element, the water,—and that in struggling with their victim they require to remain a long time under the surface of the water,—he conceived that as they possess two openings leading from the cloaca into the cavity of the peritoneum, one upon each side of the cloaca, as you see at the sides of the anus in cartilaginous fishes, that those openings very possibly admitted water into the cavity of the peritoneum, and that the animals might draw water through the cloaca, and convey it into the cavity of the abdomen for the purpose of respiring that liquid element while they were struggling under water with their prey, and that in that aquatic respiration, this abdominal ossified part might be employed. This view was conceived to be further supported by observing that there was an animal belonging to the class echinoderma—the animal called *holothuria*—which draws in water through the anus and cloaca for respiration, and conveys it through beautiful, ramified, respiratory branchiæ that extend upwards from the cloaca through its abdominal cavity. This theory of the function of the abdominal ribs is not supported, however, by any direct observations on the power which the crocodiles have of drawing in water through these apertures leading from the cloaca into the cavity of the peritoneum.

I have examined these peritoneal openings in this young gavial which is now before you, and also in these two young alligators, and I have repeatedly shown in my class that these apertures terminate in the young animals at the extremities of lengthened papillæ, projecting downwards into the cloaca; that in these animals, which are more than two feet in length, so small are the two apertures that they scarcely would admit a small pin to pass through them; that the papillæ project downwards from the ends of two funnel-shaped prolongations of the cavity of the peritoneum, so that when we direct the air from the blow-pipe downwards from the abdominal cavity into the cavity of the cloaca, it passes outwards through the two very minute projecting papillæ; but when an attempt is made to blow the air upwards from the cloaca, as water would pass in respiration, it immediately compresses the papillæ and closes up the termination of the apertures. I have not examined these apertures in the adult animals, but from what I have seen in the young animals, they appear to me to be incapable of allowing a free ingress of water for respiration in the young state. These openings may be connected with the secretions of the abdominal cavity in these truly aquatic species of saurian reptiles.

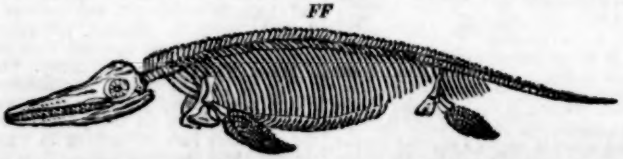
In the crocodiles, we observe that the pelvis has its three bones detached and more moveable upon each other than in most other saurians. The two expanded short iliac bones (*EE, g*) are firmly united to the two broad vertebræ of the sacrum. The two *ossa ischii* (*EE, h*) meet in front and expand like the pubic bones. Above those we observe the two comparatively small pubic bones (*EE, i*) rising obliquely, and meeting also by two expanded surfaces, which remain detached. These pubic bones scarcely approach the margin of the cotyloid cavity, and rest on a prominent ramus of the *ischium*.

In most of the lacertine *sauria*, as you see in this large monitor of Java, the iliac bones are extended much backwards, and fixed to the enlarged transverse processes of two sacral vertebræ. The pubic bones partake in the formation of the cotyloid cavity for the head of the femur, and are united together in front by a broad symphysis. The two ossa ischii also meet in front, and unite together by two broad margins, and they unite with the pubic bones in front, so as to leave two large foramina between them.

The atlantal and sacral extremities being the parts of the skeleton most remote from its axis or central column, are the parts which in all vertebrata are liable to the greatest modifications of form, and in the saurian reptiles they present endless varieties of structure, dependent on the different modes of progression of the species, whether of walking on a horizontal plane, or climbing on trees or rocks, or swimming suspended in a liquid element. The tail of the loricated or crocodilian reptiles is formed for horizontal motion through the water, and the feet are constructed both for walking on the soft muddy banks and for swimming in the water. The articulations of the extremities are less secure than in animals residing constantly on land. The *humerus* (*EE, b*) and the *femur* (*EE, k*) are both curved in the directions best calculated to give effect and strength to their various movements. The *radius* (*EE, c*) and the *ulna* (*EE, d*), as well as the *tibia* (*EE, l*) and the *fibula* (*EE, m*), are here strong bones, free to their extremities, leaving a large interosseous space, and affording a broad and secure articulation at the *carpus* (*EE, e*) and the *tarsus* (*EE, n*). The bones of the *metacarpus* (*EE, f*) and *metatarsus* (*EE, o*), and the phalanges of the hands and feet, are long, and strongly articulated at both ends, for their double use, on land and in the water.

There ar emany interesting peculiarities to be met with in the different forms of the skeleton in the existing species of this very extensive order of reptiles; but the most anomalous and remarkable forms are those which are presented to us in the ancient strata of the earth,—the secondary rocks, especially the lias limestone, which abound with the remains of extraordinary and gigantic saurian reptiles, which have swarmed in our ancient seas and estuaries. We there observe aquatic saurian animals, as the ichthyosaurus (*FF*), and the plesiosaurus, presenting arms and legs constructed upon the plan of the arms of cetaceous animals. We observe them to have had the head like that of a crocodile,

FF



as regards the form of the skull and jaws and teeth.

In the ichthyosaurus now before you, you observe there are laminæ of bone surrounding the transparent cornea, as in the eye of a tortoise, as in the eye of an American *iguana*, as in the eye of a bird. The bones of the head of the ichthyosaurus and of the plesiosaurus appear to have been united in the fixed manner which we observe in the existing crocodiles, and not as in the lacertine sauria. The teeth were also lodged in alveoli, and grooved or striated on the surface as we see in the existing crocodilida. The lower jaw, divided into several pieces, as in the existing crocodiles; and also shooting backwards beyond the tympanic bone. The tympanic bone appears in both to have been fixed; a further analogy with the existing crocodiles. The spinous processes are also largely developed in these skeletons of the ichthyosaurus and of the plesiosaurus. This corresponds with the existing cetacea as well as the large crocodilian reptiles, which are remarkable for their development of those spinous processes along the whole region of the back. The arched form of the very long and slender ribs of the ichthyosaurus gave a considerable surface and depth for the attachment and lodgment of the powerful muscles occupying the space along the sides of these spinous processes. The bodies of the vertebræ in the ichthyosaurus are deeply concave at each end, like those of a fish, and those of the plesiosaurus are almost flat at the ends, like those of a quadruped; but in both these animals, as in the crocodilian existing reptiles, the elements of the vertebræ were united by permanent sutures. The long spinous processes of the long neck of the plesiosaurus would render it impossible for the animal to curve the neck like a swan; nor would it be necessary in the dense element it inhabited, and with so very small and disproportionate a head.

You observe the clavicles anchylosed; the scapular and the coracoid bones are quite separate in both, and developed in front, in the manner which we sometimes observe in the most inferior of the existing saurian reptiles. Neither the arms nor the legs appear to have had a fixed attachment to the vertebral column. No vertebra appears, either in the ichthyosaurus or in the plesiosaurus, to have had the transverse processes enlarged into the form of a sacrum; and those posterior extremities, which were to exert considerable pressure by the impulse they gave to the water to assist the lateral motions of the tail in swimming, appear to have been as completely detached from the vertebral column as those of a fish. The arms, particularly of the ichthyosaurus, and the legs of that animal, were constructed entirely for an aquatic life, like the arms of the lowest of the blowing cetacea. The humerus in the ichthyosaurus (*FF*) is a short compressed bone, but the radius and the ulna, detached from each other and cubical, somewhat resemble in form the numerous small compressed bones that succeed in the hand,—these cubical small bones succeeding the radius and the ulna. In the ichthyosaurus you can scarcely distinguish the bones of the carpus and of the metacarpus from the phalanges of the fingers. From the extremity of the humerus it appears as if the hand immediately commenced. These bones of the hand consist of round pieces connected together by a cartilaginous substance. They have not played upon each other, as in the hand of a dugong; they have not met each other in the course of their ossification, and played upon each other so as to produce articular surfaces, as in the herbivorous cetacea and in this plesiosaurus. This is the condition which we shall again see in the fixed fin-like hands of the porpoise, the dolphin, and other piscivorous or blowing cetacea. But the hands and the feet of the plesiosaurus approach much more to the arms of the lamantin and dugong, and have more of a prehensile form in the separate bones; they have more in their lengthened and flexible structure, which would enable the animal to apply them, as the climbing herbivorous cetacea do, to the solid ground, or the inequalities of rocky coasts, than there is in the immoveable structure of the hands and the feet of the ichthyosaurus; and in the hands and arms of the plesiosaurus, we see the radius and ulna already assuming a much more distinct and lengthened form. The bones of the carpus and metacarpus assume forms more distinct from each other, as they have in the hands of terrestrial sauria and of quadrupeds. The phalanges of the fingers have here met and even played upon each other so as to have produced articular surfaces.

It is a remarkable circumstance, certainly, to observe in these large skeletons, that notwithstanding the use made of the legs in swimming, they have not produced any modification of the transverse processes in any part of the vertebral column, so as to present a distinct sacrum, as we see in almost every existing saurian reptile. But when we reflect on the uses which the existing crocodilian reptiles make in the water of their coccygeal vertebræ in swimming, we may perceive that it was an advantage to those extinct aquatic sauria to have had the whole posterior

portion of the vertebral column free from anchylosis, or fixed attachments,—free from a sacrum. The ichthyosaurus especially, in all probability, like the existing crocodilida, had its long narrow tail compressed, directed perpendicularly downwards, which it moved from side to side, having the spinous processes alone developed and much lengthened upwards and downwards on the coccygeal vertebræ.

One of the most remarkable of the fossil animals belonging to this order met with in the tertiary limestones, the calcareous slate of Solenhofen, we may still mention,—the *pterodactylus longirostris* (*GG*).

GG

It is an animal which appears to have been organised—by the solidity of the osseous frame of its trunk, and by the form of its arms, its legs, and even its bird-like neck and head—to fly through the air. It had the lengthened and slender form of head and the large orbits of a bird; and, indeed, some, as Blumenbach, have given it a place in the class of birds. It has had a place also in the class mammalia, having been thought by Soemmering to resemble a bat; it is his ornithocephalus. It has had a place also in the class of fishes, being considered to be such by Collini, who first described it. Its affinities, however, appear to connect it more obviously with the class of reptiles, than with any other vertebrated animals, and Cuvier was the first to establish its affinities to this class. This animal (*GG*) had, together with a lengthened form of the head, extremely slender jaws, which are longer than the whole trunk of the body, provided above and below with distant prehensile sharp teeth, like those of a reptile—conical recurved teeth. It had a lengthened neck as long as the trunk, and composed of seven cylindrical lengthened cervical vertebræ, admitting of the most extensive motion in every direction. The trunk of the body was remarkably short, so as actually to resemble the fixed trunk of a bird. It resembled a bird in the length and mobility of its neck, and in the shortness of its trunk and tail. The most remarkable structure which it presents, however, is in the arm and hand. It has received the name of *pterodactylus—wing-fingered* (in Greek)—from the lengthened form of the bones of one of the fingers—the ulnar or little finger—so as to have produced in the living state an appearance of the side of its body, somewhat like a bat, by the integument extending from the whole arm, as far as the extremity of the little finger, to the posterior or sacral extremities of the body. We observe that the arm was connected with the trunk, as in the existing saurian reptiles, by a scapula of a lengthened form, somewhat resembling in its narrow tapering extremity that of a bird, and by a coracoid bone extending to the sternum. The humerus, a short and strong bone. The radius and the ulna closely applied to each other, so as to admit of the adduction and abduction at the carpus, as in birds. The bones of the carpus are numerous, and arranged like those of a reptile or of a quadruped, and have no resemblance to those of any bird. The bones of the metacarpus are remarkable for their length; five in number, corresponding with the five fingers, which were never seen in a bird; they are long slender bones. Four of the fingers—the inner or radial four—terminated, apparently, by curved-sharp claws, and were short, compared with the metacarpal bones; but the fifth or outer finger, that corresponding to the little-finger of the hand, had a remarkably lengthened and strong form, to support the membrane by which it was enabled to fly. It consisted of four phalanges, each of which, when taken separately, is nearly of the length of the whole hand, so that the little-finger must have extended outwards, like one of the fingers in the wings of a bat, or the middle-finger of a bird. It could not have served for any kind of prehension, but appears to have been like one of those slender fingers, like fine wires, supporting the wings of a bat, and to have supported a membrane to enable this most anomalous and extraordinary cold-blooded animal to pursue its winged prey through the regions of the air. This is obviously an organization much better adapted to sustain a con-

tinued flight through the air in pursuit of insects or similar prey than that of the existing flying dragon, which has only a membranous parachute supported by the ribs, prolonged outwards from the sides of the vertebral column.

LECTURES ON SURGERY,

BY

MR. WARDROP.

OF THE SANGUINEOUS SYSTEM.

ABSTRACTION OF BLOOD.

(Continued from page 583.)

Quantity of Blood to be abstracted; how estimated. — Syncope; how beneficial. — Copious Hemorrhagies often not injurious. — Blood-letting, when to be repeated. — Difference in the Conditions of the Blood.

Quantity of Blood necessary to be abstracted.—In the last Lecture I endeavoured to impress on your minds more particularly three points: 1st. The peculiar incompressible state of the pulse which, in conjunctions with other symptoms, indicates the propriety of general blood-letting; 2nd. The importance of the *first bleeding*, and the almost insuperable difficulties which are to be often overcome when this first depletion has been too sparing; and, lastly, I endeavoured to point out the propriety of abstracting blood until a fainting state or syncope supervened, in all cases wherein venesection is decidedly preferable to local bleeding.

You are now naturally led to inquire,—What is this quantity of blood which it is necessary to abstract at the first bleeding, in order to produce a state of fainting? Such, indeed, is the variety of the constitution of individuals,—such the difference in the severity of disease,—and such the difference of the period when called on to treat particular cases, that you will find, that whilst in some the pulse sinks after the removal of but a few ounces of blood, in others depletion must be carried to a great extent before syncope is produced.

When employing venesection, and observing the indications which have already been pointed out in order to regulate its extent, it will be generally found that the quantity of blood which you are able to remove before fainting comes on, is, in fact, never more than is requisite for the cure of the disease. Hence a person in health usually faints from the loss of a comparatively small quantity of blood, whilst the same individual, after suffering but a few hours from active inflammation, requires to lose an almost incredible quantity,—a quantity essential for the cure of the disease,—before he falls into a state of syncope. I have also observed, with regard to leeches, that the quantity of blood which flows from their bite, varies according to the degree of congestion in the vessels of the part to which they are applied, and, likewise, according to the necessity and propriety of removing such a quantity. Hence blood often flows for many hours after the first application of leeches, whilst an equal number applied at a subsequent period, and when the disease has been greatly subdued, yield comparatively a very small quantity of blood. This difference must no doubt depend on the different condition of the vessels at the different times the leeches are applied; and so it is with regard to venesection: a large quantity of blood may be taken away at the first bleeding before syncope is produced, whilst at every succeeding operation, fainting comes on from the loss of a smaller and still smaller quantity.

If you are to be guided in the employment of blood-letting by the principles which I have been endeavouring to inculcate, you will readily conceive the differences in the quantities of blood which must be abstracted in different examples even of the same disease; and whilst, in some instances, you will be disappointed, from the smallness of the quantity which flows from the vein before syncope supervenes, you will in others be surprised at the extent to which it may be carried, and with the happiest results. Hence the difficulty of attempting to give you anything like a precise idea of the quantity of blood which patients require to lose in the treatment of particular diseases. It would be no less absurd in me to attempt to specify the quantity of purgatives or diaphoretics necessary to complete the cure of a fever! The best general notions on this subject can only be attained by observing the quantity removed in a series of cases wherein blood-letting has been judiciously employed.

In the army and navy, where inflammatory disease often assumes a very severe type, surgeons are in the habit of abstracting thirty, forty, and sometimes fifty ounces of blood at the first bleeding, and even where the treatment is thus energetic at its commencement, from one to nearly two hundred ounces of blood have frequently been removed before the disease has been subdued; but amongst other classes of the community, where the physical frame is slender, a comparatively very small quantity is found sufficient to

relieve and check inflammatory diseases. There is an observation which I would here venture to make as the result of my own experience, as well as from the many opportunities I have had of witnessing the practice of others, and that is, that of a number of persons bled for inflammatory diseases, those who have lost the largest quantities of blood, and have done so by the fewest operations, have made the most rapid recoveries; whilst those who had been more frequently bled, and had lost even a greater quantity of blood, but in small quantities, and at more distant intervals, have recovered much more slowly, and have had, more frequently, some permanent structural change of the affected organ.

Syncope; how beneficial.—The state of fainting which I have already endeavoured to point out as an unerring criterion for estimating the extent to which blood should be removed in those cases where general bleeding is most expedient, such as in inflammatory cases attended with febrile disturbance, and in dangerous congestions affecting the vital organs, syncope is, by most practitioners, taken as their guide; and though some have argued that a state of syncope thus brought on may be pernicious and even dangerous, I have never either met with, or heard others describe, a case where general bleeding judiciously resorted to caused any serious mischief from carrying it so far as to produce syncope. You ought in considering this point to discriminate between those cases where the bleeding has been employed as a curative means, and those wherein a profuse hemorrhage has taken place from wounded vessels, as from accidents, or from a partial separation of the placenta. In these the bleeding may not only produce syncope, but such may be the size of the vessels, that the blood continues to flow until life is nearly or even completely extinguished. But in the operation of taking blood from a vein, its flow can and ought to be arrested whenever syncope takes place; and surely no surgeon ever attempted to persevere in the continuance of the abstraction of blood from a person in this state. On the other hand, the state of fainting is to be considered as an index of the quantity of blood necessary to be removed for relief from the disease; and, as I have already said, you will always find that quantity to be in the ratio of the propriety and necessity of procuring it.

Though it has been stated that in no case of syncope produced by venesection, have I ever known any subsequent pernicious effects, yet you ought to be warned, that in some cases it has been carried to the very last extremity, and the consecutive symptoms have caused great alarm to the bystanders as well as to the operator. Such cases, however, must be truly rare, for accustomed as we are to hear daily of persons being bled to a great extent, yet such alarming effects are seldom heard of; and recollect also how many hundreds or even thousands of persons in this community are bled every day, and how few accidents of any description ever happen.

Case.—The most alarming effect of syncope from venesection I ever witnessed, took place in the person of a surgeon, who had an attack of inflammation of the brain, and for which he had been repeatedly bled, both by venesection and leeches, but in small quantities, without relief. His state was characterized by a dull unceasing pain in the head, which did not influence the intellectual functions, but was attended with great intolerance of light, sound, and motion. The pain becoming more intense, though his pulse was so natural in frequency and apparently so feeble as to discourage his attendant from repeating the bleeding, yet he could himself detect an incompressible feeling in it, which peculiar feeling he knew to be an unerring index of the propriety of venesection. His attendant, contrary to his own opinion, however, opened a vein, and after a very few ounces of blood were abstracted, the patient became faint, which caused the removal of the bandage. When he recovered from this state, and was able to apply his own finger to the pulse, he could still distinguish its incompressible character, and experiencing likewise the fixed pain in the head, he entreated his friend to replace the bandage. This done, a very large quantity of blood was removed before he again fainted, and the state of syncope was so complete that he remained insensible upwards of five hours, during which period those around were in the greatest apprehension, the powers of life being so exhausted, that during that long interval the pulse could not be distinguished at the wrist, and the respiration could hardly be recognised. Such however was the effect of this protracted state of syncope on the disease in the head, that when the patient awoke, as it were into life, and was able to articulate, his first expression was, that the pain in his head had vanished, and that his pulse was completely subdued. This patient's recovery was speedy and complete, requiring no further remedial means,—and in about six weeks his health and strength were perfectly restored.

This interesting and instructive case

demonstrates to you in the most striking manner how completely inflammatory disease can be arrested and subdued by producing a diminution of the heart's action; and also how, in the treatment of diseases, though a very moderate venesection may diminish the unnatural action of the vascular system, when the depletion is pursued to the extent of producing syncope, the longer the duration of that syncope is, so much the more completely will the inflammatory action be subdued. The state of collapse, as it is called, which antimonial preparations produce by their influence in diminishing the action of the heart and arteries, subdues inflammatory disease on the same principle as the abstraction of blood.

I have also, in a few instances, observed persons recover from inflammatory affections, whose vital powers appeared so reduced as to render their condition apparently hopeless, but which low condition seemed to be the very means of arresting the diseased action.

Case.—A girl, about ten years of age, had suffered severely from hooping-cough and congestion in the brain, for which the depletive system had been adopted; but such was ultimately the state of debility to which she was reduced, that I found her apparently lifeless, and surrounded by her parents, bewailing her approaching dissolution! Without a shadow of hope I poured a spoonful of wine into her mouth, and instructed her attendants to give beef-tea and wine alternately, and at short intervals. Considering any further professional attendance unnecessary, and calling the following day on the other surgeon who had visited the patient, requesting him to get permission to examine the body, think of my surprise, when with a smile he informed me that the patient was doing well, having recovered every hour after the exhibition of the cordials.

Cases of this description ought to teach us the propriety of never ceasing to give assistance, however slender it may appear, for the purpose of prolonging life, and I am confident it not unfrequently happens that the state of great debility or collapse in which you may occasionally observe those who have severely suffered from febrile diseases and large depletions, becomes the very means of arresting disease. Such an effect is most to be anticipated, even when life has been apparently nearly extinguished. In the diseases of children, more particularly when the vital powers have been greatly exhausted, and when the child refuses his food, I have frequently known instances where the infant was in this state, and its life was despaired of, and when no endeavours were made by the parents to prolong its existence—that the vital powers were resuscitated by forcibly separating the teeth, and forcing cordials into the mouth, always taking care in such cases that the fluid is conveyed by the spoon to the root of the tongue. The truth of the common maxim, that "whilst there is life there is hope," is here strikingly exemplified.

Copious Hemorrhagy often not Injurious.

From a general impression that blood-letting ought not to be employed but in diseases of a serious character, and that it is a remedy from which much mischief may ensue, if carried to too great an extent, or employed in improper cases, it is, no doubt, on many occasions, too tardily resorted to, and too sparingly used, whilst we every day see medicines, the effects of which may be highly injurious to the constitution, fearlessly and carelessly exhibited. That this dread of blood-letting is not founded on sound observation, is demonstrated by noticing the effects of spontaneous hemorrhagies—of profuse bleedings, so often the consequence of injuries, and also in diseases where they have accidentally taken place.

Alarming hemorrhagies sometimes ensue during, or after, operations; and I have already remarked, that those patients recover much the soonest, in whom such a large quantity of blood has been lost.

In the spontaneous hemorrhagies from mucous membranes, as from those of the nose and rectum, it is extraordinary what large quantities of blood persons lose with no other sensible effect, when they have no organic disease, than the production of more or less debility.

Case.—A man was admitted into the Hospital of Surgery with dropsy, which he expected would be relieved by tapping. A few hours after his admission, he was seized with bleeding from the nose, and although his pulse was feeble and his skin cold, having lost not less than two quarts of blood, I bled him at the arm; the bleeding from the nose then ceased. But the particular circumstance in this case was, that notwithstanding this great loss of blood, his pulse on the following day became firm and wiry, and was accompanied by pain in the abdomen; he was therefore again bled at the arm. On the second day the pulse rose, and he was bled a third time, after which he was tapped, and got a large dose of calomel and opium. This treatment had the effect of completely mitigating his sufferings. Calomel and squills were then given, until his gums became tender. He left the hospital fourteen days after his ad-

mission completely relieved, and a few weeks afterwards I saw him in perfect health.

The quantity of blood which some persons lose from hemorrhoids, is also extraordinary. I have known instances of delicate females losing daily, on an average, from half a pint to a pint of blood for many months, and even years,—a quantity which, when compared with the largest quantities of blood ever removed by artificial means, appears astonishing; and the slight disturbance of the system, which such profuse hemorrhage occasions, seems indeed unaccountable.

Similar effects of the loss of large quantities of blood are exemplified in cases of varicose veins accidentally ruptured, and from which immense quantities of blood have escaped, without any perceptible injurious consequences. Petit particularly mentions such cases.

Hemorrhage to an enormous extent sometimes happens on the field of battle, wounded soldiers having been often left apparently lifeless, and, as has been already observed, such wounded men recover with unusual rapidity, from the complete check which is thus given to consecutive inflammation.

Very extraordinary, also, is the quantity of blood that females sometimes lose from the partial separation of the placenta, and without any other bad effect than temporary debility. Women under such circumstances have been known to lose several quarts of blood in a few hours!

These facts, taken collectively, prove to what an extent blood may be lost, without being followed by serious consequences. And the practical conclusion to be deduced from them is, that blood-letting may be extensively employed, when duly managed, for the cure of disease.

Blood-letting, when to be repeated.

Though, in the generality of cases, much depends on the full quantity of blood being taken at the first bleeding, as well as having recourse to blood-letting in the early stages of disease, still there are others wherein the complaint can only be cured by the frequent repetition of venesection.

The frequent repetition of blood-letting is rendered necessary from different causes. It becomes requisite in some instances, in consequence of a fainting state coming on after only a small quantity of blood has been abstracted, and, in other cases, when the pulse sinks after a very moderate quantity has been removed.

But when, from whatever cause, the quantity of blood is small, it ought to lead to the more close watching of any return of the symptoms, and prepare you to repeat the bleeding.

Now it frequently happens, that on the repetition of the venesection, the cause which prevented the first depletion being sufficiently copious, no longer exists, so that at a future bleeding any quantity of blood which may be deemed necessary, can be readily abstracted.

But the most frequent cause for repeating venesection is, that to whatever extent the first bleeding may have been carried, it is seldom that one depletion is sufficient to check the progress of a severe attack of inflammation, particularly in an important and vital organ.

Always bear in mind, that the more complete the syncope has been from the bleeding, and the sooner the second bleeding is performed after the pulse has begun to rise, the less will be the quantity of blood necessary to be taken away at the second bleeding, as well as the aggregate quantity for the complete cure of the disease. After the first bleeding, therefore, the pulse ought to be closely watched, due attention being also paid to the other symptoms; and whenever the vascular system has begun to recover its vigour, which may take place in three, six, or twelve hours, then replace the bandage on the arm, wipe away the coagulum in the wound of the vein, and allow the blood to flow, until the strength of the heart and arteries is again subdued.

Different Conditions of the Blood.—There are appearances which blood presents, both whilst it flows from the vein, and after it has been kept some time and allowed to coagulate, which have been considered as affording criteria for regulating the quantity of blood to be withdrawn, and also for the repetition of venesection.

In violent inflammations, the blood usually flows from the wound in the vein with great force; whereas in other diseases the stream is comparatively slow. In the first case the bleeding ought to be as profuse, as in the latter it should be limited.

The colour of the blood as it flows from the vein, also varies in different examples of disease, being in some very florid, or of a scarlet red, and in others of a crimson red or deep purple colour. The first state of the blood indicates the propriety of bleeding, whilst the latter points out a state of the system where but a very moderate depletion is admissible.

The appearances which blood assumes after it has been allowed to stagnate, are also supposed to afford criteria for the repetition of venesection. The appear-

ance of a buffy coat, and the comparative quantities of crassamentum and serum have especially been considered as an index of the existence of inflammation, and of the propriety of blood-letting.

Whilst all these circumstances are mentioned, I must at the same time repeat what was stated in a former lecture, when explaining the different changes of the blood, that these can never be a criterion for estimating the quantity proper to remove, whilst the blood is flowing from the vein, neither can any appearance of the blood after its coagulation guide you in repeating the venesection. The appearance of the buffy coat has been chiefly dwelt on as an important character of the inflammatory diathesis; but as I have also observed, the buffy coat is not to be considered either as a certain test of inflammation, or a safe index of the propriety of bleeding. There is usually no appearance of the buffy coat in blood removed from persons affected with violent inflammations until the latter stage of the disease, and at the very time when the further abstraction of blood would be pernicious; and, on the other hand, in many diseases where blood-letting is unnecessary or even hurtful, the buffy coat may be occasionally observed.

Before concluding this subject, I have yet to point out the circumstances which indicate when blood-letting should be discontinued, and also the injurious effects which sometimes arise from the loss of blood.

PARALYSIS OF ONE SIDE OF THE FACE.

To the Editor of THE LANCET.

SIR,—In your journal for the 11th of January, I observed the following paragraph:—"'I see they say in THE LANCET, or some other journal, that Dupuytren is ill, and that he has had paralysis of one half of the face. Now I never knew a case of this kind in which the patient did not ultimately recover. It does not arise from injury to the cerebrum, but from an affection of the portio dura.'—*Mr. Brodie at St. George's.*"

Having some time ago been affected in a similar manner, I forward you an account of my own case, the termination of which accords with the remark of Mr. Brodie, and substantiates the opinion of Sir C. Bell, given in his admirable work on the "Nervous System;" wherein he relates the case of a clergyman, one half of whose face was paralysed from exposure to the draught occasioned by the open window of a stage coach.

Yours very respectfully,
WM. RAYNER, M.R.C.S.
Bury, Lancashire, Jan. 21, 1834.

CASE.—Jan. 11th, 1824, I was aroused at an early hour in the morning by an application to visit a female in labour, who lived at the foot of Blackstone-Edge, a distance of four or five miles from Rochdale, my then place of residence. The case being one of emergency, and being obliged to dress in haste, I merely wrapped a thin cravat around my neck, neglecting the other usual coverings. On my journey I was exposed to a cold sharp wind, from which I felt much chilled about the face and neck. After being in the house an hour the labour terminated, when I remained to partake of tea with the female attendants, during which some anecdote was related that excited laughter, when, to my great surprise, I suddenly felt a peculiar sensation, and a numbness, in the left side of my face. I immediately turned to a mirror, and to my dismay perceived the whole of that side drawn completely awry, and presenting a most ludicrous appearance. Prior to this attack I had enjoyed uninterrupted good health; age 25, person spare, and habits strictly abstemious. For nearly two years the left angle of the mouth had so completely lost its power, as to render it difficult during mastication to retain the food when on that side. I was also incapable of perfectly closing the eyelids of the affected side. I used a variety of remedies suggested to me by professional friends, without benefit, excepting what I derived from simple friction, which I daily employed with the flesh-brush, and which I have every reason to believe was advantageous. Ten years have now elapsed since the attack, and I am perfectly recovered, having suffered the paralysis about eight years, without the slightest symptom of a relapse.

Reflecting on this case I should say, that the cause was not dependent either on disease of the brain or the digestive organs, but that it originated solely in an idiopathic affection of the portio dura, arising from exposure to cold.

THE BARON DUPUYTREN has, since his journey to the South, recovered so happily and speedily, that a few weeks since he commenced taking his pleasure in Naples, and is now anxious to be again receiving his daily morning roll and pulling refractory patients by the hair in the *Hôtel Dieu*.

ORGANIC STRICTURE OF THE RECTUM.

LETTER FROM DR. O'BEIRNE.

IN REPLY TO THE REMARKS OF MR. SALMON.

On the existing Doctrines respecting the Morbid Conditions of Organic Stricture of the Rectum; with Arguments to show that those Doctrines are erroneous, and are made the foundation of an improper Course of Treatment; and a Statement of Facts in Support of the new Views of Defecation, lately promulgated by Dr. O'Beirne.

To the Editor of THE LANCET.

SIR,—I find that Mr. Salmon's reply to me contained in THE LANCET of December the 7th, page 407, is a general one; and that by embracing a great variety of points, it is calculated to divert the mind from the subject of organic stricture of the rectum, upon which we are mainly at issue; and which we had mutually agreed to leave to the decision of two distinguished members of the profession. This course may suit his purposes, but will not answer mine. For the present, therefore, my rejoinder shall be confined to fixing your attention, and directing my observations exclusively, to the matter *in arbitrio*. But I reserve to myself the right of seizing an early opportunity to answer the other parts of Mr. Salmon's reply.*

The exact nature and practical bearing of the difference between Mr. Salmon and me, on the subject of organic stricture of the rectum, do not appear to be fully understood, either by Mr. Salmon himself, or, with the exception of Mr. Cooper, by

* Before I proceed further in this matter, I regret to be under the necessity of making known, and animadverting upon, some of the circumstances under which reports upon the subject in question have been laid before the profession. The circumstances are these:—By mutual consent, Dr. James Blundell and Mr. Bransby Cooper became umpires on the difference between Mr. Salmon and me; yet week after week and month after month continued to elapse without bringing any decision from those gentlemen. Such delay, in deciding a matter so easy to be determined, induced me to believe that their investigations could not be very unfavourable to me, and that there was a hitch somewhere. Accordingly I wrote to Mr. Cooper, calling upon him for an explanation, and urging him to come to some decision on the question. His answer was to this effect:—That he had waited for, but had not received, Dr. Blundell's opinion; that his own had been long made up, and that it was decidedly in my favour. Shortly after receiving this intelligence, Mr. Oldknow's letter appeared in THE LANCET, and, in the course of a few days more, Mr. Cooper inclosed to me a statement from Dr. Blundell, the same in all respects as that which you and the editors of the *London Medical and Surgical Journal* have published; and I found that this statement contained facts which appeared to me to be perfectly conclusive as to the soundness of Mr. Cooper's decision. I had, therefore, these favourable documents in my possession so early as the middle of last November, and might easily have anticipated my opponent by many days, in placing the result before the profession. But, instead of doing so, or taking any advantage of Mr. Salmon, I again wrote to Mr. Cooper and submitted to him the necessity of shifting the preparations marked *A* and *B* in their whole length, before either he or Dr. Blundell could fairly come to a final decision. But it would appear that Mr. Cooper was prevented from making this fair and considerate proposal, or interfering further in the affair, by a step which Mr. Salmon had taken in the mean time, namely, requesting Mr. Stanley and Dr. Ryan to inspect his preparations, and give their opinions on the contested points. Now, although these gentlemen are perfectly unexceptionable, and their characters and opinions above all suspicion of partiality, I submit to you and to the profession, that this step was a positive breach of a compact mutually agreed to, and acted upon for three months; and, on the part of Mr. Salmon, anything but respectful or complimentary to either his own friend or to mine. He tells you, indeed, that he took this step in consequence of "finding they" (Dr. Blundell and Mr. Cooper) "differed so materially." But was it then, or is it now, the fact? Dr. Blundell is silent respecting any such disagreement. Mr. Cooper says expressly, that his "observations bear a great resemblance to those made by Dr. Blundell upon his inspection of the same preparations:" and I not only cannot see that any such difference exists between those gentlemen, but pledge myself to demonstrate that they agree perfectly with each other. Let me suppose, however, that the umpires originally appointed differed as widely as the poles are asunder, and that this difference justified my opponent in calling in the aid of others without my knowledge or consent, would it not have been fair, and no more than fair, to have invited Mr. Cooper to be present at the inspection made by Dr. Ryan and Mr. Stanley? Yet this was not done; and that it ought to have been done, will be evident from the fact, that, in consequence of the subject of organic stricture of the rectum being but very briefly touched upon in a part of one of the notes in my work, Mr. Cooper was obliged to call upon me for various explanations and definitions, before he became thoroughly acquainted with the exact nature of my views on the subject, or could come to a decided opinion on the question before him. It is clear therefore, and it shall be my business to make it still clearer, that on making their inspection, Mr. Stanley and Dr. Ryan had but one side of the question fairly placed before them. The truth is, that Mr. Salmon, by being on the spot, has had great and obvious advantages over me, and has not been at all oversqueamish or punctilious in availing himself of them. In this controversy also he enjoys, and he knows it, advantages of another description, and of no mean importance. He is not, like me, defending his own views, but those which have been long and universally received; and, consequently, he can count upon seeing ranged on his side that great body of readers who will not be convinced until they can no longer offer opposition; while I can only count upon the comparatively few who are ready to embrace *truth*, let her come in ever so homely a garb, or from ever so humble a quarter. But let it not be supposed that I mean to deprive my adversary of any one of these advantages, whether natural or acquired; or that it is my intention either to object to, or shrink from, any evidence that he has adduced. No, the cause of humanity and science spurns all such dexterous expedients, and demands to be pleaded boldly, frankly, and without subtlety or evasion of any kind. In this spirit I enter on my undertaking.

any other of the gentlemen who have been engaged in deciding on that difference. It is indispensable, therefore, that I should commence by removing all obscurity in these respects. With this view I shall exhibit,

First, the ideas that are now, and have long been, held, respecting the morbid conditions constituting organic stricture of the rectum.

Secondly, the various points in which those ideas appear to me to be false.

Thirdly, the danger and impropriety of a mode of treatment which has been suggested by these false ideas.

Fourthly, the peculiar views which my investigations have led me to entertain respecting the formation of organic stricture at the upper extremity of the rectum, —the manner in which the morbid action subsequently extends upwards and downwards,—the different kind of strictures produced by its extension in the latter direction,—the sources of false impressions communicated when the finger is employed to examine these strictures,—the manner in which these strictures require to be treated.

By proceeding in this natural and orderly manner, I expect to succeed in placing the whole subject in clearer, fuller, and much more useful points of view than the peculiar nature of the plan of my work admitted.

1. First, then, with respect to the received ideas of the morbid conditions constituting organic or permanent stricture of the rectum. The nature of these ideas cannot be more fully or fairly exhibited than by the following extract from the work of the very latest writer on the subject:—"Permanent stricture of the rectum," says Mr. Herbert Mayo, "consists in a partial thickening of the submucous coat of the bowel, and of the adjacent cellular texture, through which means a smooth ring is formed, generally from a third to half an inch in depth, which projects into and narrows the channel. Sometimes the thickening does not include the whole circle of the intestine, but a segment only. It is presumable that this thickening results from chronic inflammation. The ordinary seat of stricture of the rectum is from two and a half to four inches from the orifice of the gut. But sometimes it occurs at a greater distance, at six to seven inches for example."—*Observations on Injuries and Diseases of the Rectum*, p. 165-6. Lond. 1833.

Two questions naturally arise out of the statements contained in this extract. The first is, Can we account for the occurrence of such morbid conditions by any peculiarity in either the structure or the function of the rectum? I should think not. On the contrary, as the disorganising process is known and admitted to commence in the submucous and cellular tissues of the intestine, it is only necessary to reflect, that these tissues form a very loose and delicate connexion between the mucous and muscular coats of the bowel, in order to see that when they become the seat of such a process, the inflammatory action should necessarily spread equally in all directions, and, consequently, cause uniform, not partial, thickening of the parietes of the gut. It may be contended, perhaps, that the folds of the mucous membrane of the rectum favour the production of such circular septa or projections as Mr. Mayo and other authors describe. But this is a supposition which will not bear even a moment's examination; for as the folds in question are almost wholly found in the longitudinal direction, it follows that the septa or projections supposed to be thus formed, should not be found observing a circular, but a longitudinal course; a course which no author has described them as taking. Hence, it appears, that the mucous membrane cannot have anything to do with the production of such morbid growths; and it is perfectly obvious that the peritoneal coat is quite out of the question. It only remains, therefore, to consider how far the action of the muscular coats of the rectum are capable of contributing to the formation of the morbid projections or septa under consideration. This brings me to a theory which Mr. Salmon has advanced. After saying, that he "merely contends for the existence of permanent stricture in the lower part of the rectum," and admitting, that distinct, thickened, and shelf-like projections are exceedingly unusual in the two upper-thirds of the rectum, he asserts that "circular stricture will, from the redundancy of the circular fibres, be most commonly within reach of the finger." But if this reasoning were correct, it should be equally applicable to a higher portion of the bowel, where the longitudinal fibres are known to predominate, and where, be it observed, he admits permanent stricture to be of such rare occurrence. Moreover, it is quite obvious, that the only basis on which this theory rests, is the supposition, that the longitudinal and circular muscular fibres of the rectum act separately and independently of each other; a supposition as perfectly original as it is untenable. For example, when these two muscular layers are compelled to relax by the descent of feces, who is it that would dream of these layers not acting simultaneously? Again, when these layers

are in the act of expelling the feces, who is it that would dream of their not contracting together? Yet the only actions of these layers are relaxation and contraction. Away then goes Mr. Salmon's theory; and so will go, I have no doubt, every other that may hereafter be constructed with a similar view; for although I have shown, that the two orders of muscular fibres at the upper extremity of the rectum, often combine to produce spasmodic, and eventually organic, stricture at that point, I can find no similar principle to account for the same order of fibres producing the disease lower down in the intestine. I repeat, therefore, that there is nothing observable in either the structure or the functions of the coats of this bowel, to explain the occurrence of such partial thickening of its walls, or of such morbid growth from its walls into its cavity, as are usually described by authors.

2. So much for the first question that has arisen out of the statements contained in the extract which I have given from Mr. Mayo's work. But the second question which they suggest, comes more home to, and at once grapples with, the subject in debate. It is simply this:—Are the statements just referred to, sustained by the evidence of pathological anatomy? In answer to this question, I have only to say that the views which I have advanced respecting the anatomy, physiology, and pathology, of the coats of the rectum, induced me to attempt its solution, by examining carefully all the anatomical museums of this city. Two of these are extensive, and have been collecting for more than half a century; three of them, although necessarily smaller, contain a great variety of very valuable preparations; and I shall now state both the morbid changes I have not observed, and those which I have observed, in such specimens of organic stricture of the rectum as these collections enabled me to examine.

Except at the upper extremity, I observed no partial thickening of the parietes of the intestine at one or more points, and leaving healthy spaces between them; nor anything like smooth rings, or segments of rings, of any depth, or projecting to any extent into, or narrowing the cavity of, the bowel; and, as to the morbid changes which I have observed, they are easily described, and range themselves under the three following heads:—

First. Thickening of the parietes, and narrowing of the cavity, limited to a small portion, often but a few lines, of the upper extremity of the rectum;

Secondly. A greater degree of disorganization at the same point, and extended, but with much less thickening of the parietes, and far less narrowing of the cavity, a short distance upwards into the sigmoid flexure, and downwards into the rectum;—

Thirdly, a greater or less portion of the sigmoid flexure thickened and ulcerated, and all that great portion of the rectum above its pouch thickened in its parietes, and contracted in its cavity.

These are the only kinds of organic stricture I have found, and, of course, the only kinds of which I do, or shall, admit the existence, until satisfactory evidence to the contrary is produced. The first of these may be termed "primary stricture," because I have shown that it is the first formed, and produces the others. The last may be called "long stricture," from its being often seen presenting appearances closely resembling those of the long cartilaginous stricture of the urethra. And the second may perhaps be fairly named "transition stricture," as the parts are merely in the act of passing from the condition of a primary to that of a long stricture.

You have now before you all the facts and reasoning upon which I have adopted such peculiar views of organic stricture of the rectum, and you will perceive that the grand difference between Mr. Salmon and me upon this part of our controversy, consists in my denying that partial thickening, rings, segments of rings, or rings of any depth whatever, occur at any point below the upper extremity of the rectum, where, however, and let it be clearly understood, I admit the existence of a septum, with an opening of greater or less diameter in its centre. But I feel that all the facts, and all the reasoning that I have brought to bear upon the subject, will be insufficient to convince many of your readers, particularly those who have not read my work, if I do not endeavour to remove one great and very natural obstacle to their conversion—namely, the distinct feel of a ring, or a succession of rings, communicated on such occasions to the finger in examinations per anum. I shall endeavour to prove, therefore, that this feel, however distinct and imposing, is altogether deceptive. For this purpose I shall consider the matter under two opposite but natural points of view.

Let me suppose, that such an examination *per anum* is made by a surgeon, who believes, as all have hitherto believed, that the rectum, in its natural state, communicates freely, and at all times, with the sigmoid flexure, and that the parietes of the rectum stand wide open and apart. He

feels that the pouch of the intestine is so open, that he can roll his finger freely about in it; and that, on pushing his finger higher up in the direction of the bowel, its point enters a narrow opening or ring. But what idea does he form of the nature of this ring? The most natural that can be; that it is formed by an opening in the centre of a septum thrown across the cavity of a gut, which he supposes to be always in a dilated and open state.

The feel communicated to his finger being too distinct and perfect to be doubted, and the partial circular thickening which he has so often seen at different points of the urethra, are all circumstances which combine to fix him in this opinion. But let me now place the matter in a different point of view. It seems to be generally admitted, that I have succeeded in establishing the following facts:—1st. That the rectum is much more muscular, and endowed with a higher order of motific nervous influence, than any other portion of the alimentary canal, the œsophagus alone excepted;—2ndly, That, as an inevitable consequence of this organization of the rectum, instead of being in a dilated state, and communicating freely, and at all times, with the sigmoid flexure, the whole of that great portion of the intestine comprised between its upper extremity and its pouch, is so firmly contracted as to cut off all communication with the flexure, except for the few seconds that the alvine contents are in the act of passing from the latter into the former, and being expelled from the body;—3dly, That in consequence of having the middle and posterior divisions of the levator ani muscles inserted into it, and at opposite points, the pouch of the rectum is constantly open and dilated. Such being the real state of the facts, we are enabled to arrive easily at the true view of the matter. Thus, even in the natural state, when the finger enters a narrow opening above and leading from the pouch, and conveys this feel of entering a ring, we recollect that this opening is nothing more than that which is necessarily formed by the inferior extremity of the contracted portion of the intestine. Again, when the finger is pushed through this opening or ring, no matter to what height it may be urged, its point there receives a similar impression; because the parietes of that part of the bowel immediately above the point of the finger remain contracted, and, of course, convey the feel of a septum, with a narrow opening in its centre, thrown, as it were, across the cavity of the intestine. Thus we perceive, that the feel of a succession of rings or strictures, varying in number according to the manner in which the finger is introduced, may be communicated in examinations of even the healthy rectum. How much more imposing, therefore, must not this feel be in cases where the mucous and muscular coats of this bowel are morbidly thickened, particularly in those to which I have given the name of "long stricture!" So imposing, indeed, does this feel appear to have been in its nature, that, unless by its influence, it would be difficult to explain how such projecting ring-like strictures have happened to be spoken of as ordinary occurrences, without any attempt being made to establish their existence by the evidence of pathological anatomy. But as I have already shown that this, the only unerring evidence, is opposed to their existence, it is to be presumed that the influence of the sense of touch, which I have shown to be in this instance so very fallacious, will cease to sway the mind against the reception of views, which appear to be as simple and natural as they are obviously well founded.

3. The practical bearing of the foregoing observations is very important. It is this:—To show the unscientific and dangerous nature of a practice, which has evidently been suggested by the erroneous notions hitherto entertained respecting organic strictures of the rectum; and which consists in dividing such strictures as are within reach of the finger, in the direction of the sacrum, and at other points of their circumference. This practice appears to have originated with Wiseman, and is recommended by Copeland, White, Sir Charles Bell, Calvert, Salmon, and Mayo. It is evident also from their works, that the object of these authors is to divide an annular septum, which they suppose to project from the walls into the cavity of the intestine. But, if any doubt be entertained on this point, it will be quickly removed by turning to page 334 of Sir Charles Bell's *Treatise on the Diseases of the Urethra, Vesica Urinaria, Prostate, and Rectum*, where this distinguished surgeon will be found directing us "to notch the *membrane* in several places," and then adds that "this will be more effectual, and attended with less hazard, than one deep cut across the *septum*." Is it not perfectly clear, then, that it is the wall of the rectum, and not a membranous septum traversing its cavity, that is divided in this operation? If, as it is asserted, the operation enables us to introduce a bougie, which could not be otherwise introduced, is it not equally clear that it does so by enabling the instrument to lacerate the bowel? Will not the same cut that divides the stricture, particularly if that cut be in the direction of the sacrum, also divide the hemor-

rhoidal arteries and veins, and cause dangerous hemorrhage? If my views and statements be correct, all these questions must be answered in the affirmative, and the operation itself declared to be both useless and highly dangerous. But it is not merely on these theoretical principles that this opinion may be maintained, for the following extract, which conveys a very favourable idea of the author's candour and accuracy of observation, will be sufficient to show that it is also supported by the results of experience. "The division of a stricture of the rectum," says Mr. Mayo, "is not entirely free from risk of one description. I divided in a woman a stricture of the rectum, situated within three inches of the anus. In this case, as in the preceding, the division was made in a direction towards the sacrum. The wound bled at the time, but not to an extent to make me apprehensive of its return. A few hours afterwards, however, very serious hemorrhage supervened. This was arrested by the introduction of a pledget of lint, saturated with a strong styptic solution, which was applied to the divided stricture. But the patient had lost so much blood, that I thought it not improbable that I should be compelled to look for and tie the bleeding vessel on account of a return of hemorrhage. Except, therefore, under peculiar circumstances, I am not disposed to recommend the division of a stricture. The operation is painful; it does not render the use of the bougie unnecessary; and it is liable to be attended with a considerable loss of blood." —*Observations on Injuries and Diseases of the Rectum*, p. 173-4.

4. This extract contains matter bearing too strongly on the subject to require further comment. I shall pass on, therefore to the consideration of the best mode of treating organic stricture of the rectum, whether situated within reach of the finger or not.

In what I have called "long stricture," the cavity of the rectum is uniformly narrowed, and its coats are thickened, from the upper extremity to the pouch of the intestine; but there is nothing even approaching to distinct, thickened, and shelf-like projections found in the interior of the bowel. In cases of this kind, it is obvious that the stricture must always be within reach of the finger; and that the most natural and efficacious plan of restoring the natural caliber and condition of the gut, is by gradual and cautious mechanical dilatation, and thus causing absorption of the new matter deposited between the coats of the intestine. Hitherto the bougie has been the instrument chiefly employed to effect this object; but it is subject to several weighty objections. It is too inflexible to follow the course of the rectum, or to be introduced with safety to the necessary height; and experience shows that it cannot be retained long in the bowel, without great inconvenience, if not pain, from its weight, and the unnecessarily strong pressure which, as a solid body, it exerts on the sides of the gut. On the other hand, in a firm but flexible gum elastic tube, of a size proportioned to that of the stricture, sixteen inches in length, and open at both ends, we have an instrument free from all these objections; for, although hollow, and comparatively lighter than the bougie, it exerts as much pressure as is necessary, and, when introduced high enough to enter a short way into the sigmoid flexure of the colon, which should always be our object, it can be retained in that situation for several hours, and with very little inconvenience. This last advantage is one of the highest importance to the end which we have in view. I know the fact to be as I have stated it; and it may be easily explained thus: When the point of a bougie enters the sigmoid flexure, the instrument is grasped a little lower down by the upper annulus or entrance of the rectum, and so as to prevent the escape of either flatus or fluid feces, with which, be it recollected, the whole of the colon is almost always distended in cases of stricture of the rectum. In other words, this state, instead of being relieved, is exasperated by the introduction of such a foreign body as the the bougie, and the natural consequences are, efforts at its expulsion, while, on the contrary, the introduction of a *tube* gives exit to the accumulated gaseous and liquid contents, and not only places the patient, but preserves him, in such a state of comparative ease, that he is scarcely sensible of inconvenience from the presence of the instrument.

We see, therefore, that we have only to proceed upon these principles, in order to be enabled to make a successful impression upon the "long stricture" of the rectum; and, *a fortiori*, upon the "primary" and the "transition" species.

I have now put you fully in possession of my general views, and the precise nature and practical bearing of the difference between Mr. Salmon and me, on the subject of organic stricture of the rectum; and, by having done so, have placed myself in a better position to meet Mr. Salmon's observations, and enter on a close analysis of the statements which Drs. Blundell and Ryan, and Messrs. Stanley and Cooper, have laid before the profession.

Commencing, then, with Mr. Salmon's observations, that gentleman accuses me of "equal candour and incaution" in proposing such terms for the decision of our difference. I thank the gentleman for his acknowledgment that my conduct has been so fair and straightforward in this respect. But, when he speaks of my incaution, he will find that he has attempted to seize the laurel before he has obtained the victory. Again, in alluding to my denial that distinct, thickened, and shelf-like projections exist lower down in the interior of the rectum than the upper extremity of the intestine, my opponent says,—"Dr. O'Beirne is but ill acquainted with those matters, else he would know that it is exceedingly unusual to find any other result in the two upper thirds of the rectum, how much soever it may be contracted, than a uniform thickening of its parietes, from deposition in the cellular tissue, between the mucous and muscular coats, by reason of the longitudinal fibres preponderating at those points, while circular stricture will, from the redundancy of the circular fibres, be most commonly within reach of the finger." I have already exposed the weakness of the theory with which this passage concludes, and shall not now stop to notice the *courteous* terms with which it commences, but proceed to consider the several admissions it contains. So then, according to him, organic strictures, and, of course, shelf-like projections, are exceedingly unusual in the two upper thirds of the rectum. This admission goes nearly the whole of the way towards acknowledging the correctness of my opinions. But Mr. Salmon is rather late in claiming acquaintance with the fact itself, for, so far from its being mentioned either in his work, or in his lectures, he represents the matter very differently. Thus, at page 23 of his *Practical Essay on Stricture of the Rectum*, he says, "In the greater number of the cases which have fallen under my observation, the stricture has been situated between five and six inches from the anus;" that is to say, situated in the two upper thirds of the rectum, where he now denies their existence. He cannot but know, also, that he is directly at variance with all authors, myself alone excepted, when he admits that distinct, thickened, and shelf-like projections are so exceedingly unusual in the two upper thirds of the rectum. It is evident, therefore, that this opinion is of very recent date, and that it has been forced upon him by the facts and arguments that I have adduced, aided by the results of the close examinations to which his morbid preparations have been submitted.

In fact, the admission which I have just considered, and which it is obvious this controversy has extorted, is of such a nature, that my opponent is necessarily obliged to go still further, and to say, that he "merely contended for the existence of permanent stricture in the lowest part of the rectum," that is—at, or very little above, the top of the pouch of that intestine. But the context clearly shows, that he does not deny the existence of this kind of stricture at the upper extremity of the bowel, a point upon which we have not had any difference. It only remains, therefore, that I should show, that organic stricture is never limited to any point within reach of the finger, in order to establish the whole of my views, and I expect to be enabled to do this, by the following analysis of the various reports which have been made upon the subject.

Dr. Blundell describes two preparations, marked *A* and *B*. Commencing with his description of the first of these, and taking it, paragraph by paragraph, and comparing it with the descriptions of Dr. Ryan and Mr. Stanley, the following is the first paragraph to be examined.

"In this preparation (*A*) there is a distinct annular contraction, as if produced by a packthread drawn with moderate tightness round the bowel, closing in so much as to preclude the insertion of the little-finger. This contraction is situated about two inches and a half above the outer verge of the anus, rather less than more, and is decidedly within reach of the fore-finger, independently of the help which might be derived from urging." Mr. Stanley, under the head "1. Specimen.—From a female," informs us that the outer surface of the intestine presented a circular indentation of only one line in breadth, and to a depth sufficient to reduce the cavity of the bowel to about one-third of its natural size. Now, let me ask, are such appearances on the *external* surface of the bowel the natural consequence of *organic* stricture? No; even my opponent admits that this form of the disease is produced by the deposition of new matter between the mucous and muscular coats, and that this new matter, instead of approximating, separates these coats from each other, and, consequently, *opposes* the formation of anything like an indentation on the external surface of the gut. It is certain, also, that we know of no instance in which organic stricture has been found, in any situation, so attenuated as to be scarcely one line in breadth. But Dr. Blundell's next paragraph supplies another objection to considering this preparation as one of organic stricture.

"Immediately above the stricture," says

Dr. Blundell, "an incision has been made in the side of the rectum, the thickness of which is visible, and exceeds one-eighth of an inch, and the inner membrane, also exhibited, seems evidently in one part of it to be destroyed by ulceration, and in another, where it remains unbroken, its structure is changed, being roughened and thickened." From this description, it cannot be clearly collected that there was change of structure either *at*, or *in* the point of contraction. Mr. Stanley only mentions it as being observed above the contraction. Dr. Ryan, however, describes "change of structure *in* and about the contraction." But, on the other hand, Mr. Cooper, in a letter to me, states, that the contraction is evidently not the result of change of structure." In fact, it is evident that this point could not be ascertained by merely inspecting the free surface of the mucous membrane, and that the only way of deciding it, was to cut through the annular contraction, in order to see whether there was any deposition of new matter in the loose tissue connecting the mucous and muscular coats. But this was not done; and if it had been, I have not the least doubt but that no such deposition would have been found, and that the annular contraction would have disappeared.

Upon the whole, it appears to me, that the appearances described by Doctors Blundell and Ryan, and Mr. Stanley, fully bear out Mr. B. Cooper in pronouncing this preparation to be one of spasmodic stricture, a variety of the disease upon which Mr. Salmon and I have not had any difference.

"In preparation *B*," says Dr. Blundell, "apparently the entire rectum, and a part of the sigmoid flexure of the colon, together with the womb and vagina, throughout the greater part of its length (both *in situ* with respect to the rectum), are carefully exhibited. *In this preparation either the entire rectum, or certainly all except the upper portion, where it is continued into the sigmoid flexure of the colon, is exceedingly thickened, and so contracted throughout the whole of the thickened part, that the little-finger, especially in the inferior portion of the rectum, could not be passed along it without some violence.* Where there has been most deposit, the coats of the bowel exceed in thickness three-fourths of an inch. Various sinuses run in various directions, one apparently opening into the vagina."

"The uterus and vagina, lying *in situ* upon the rectum, seem to mark distinctly that part of the rectum which is most contracted and thickened, and which, as described, is the inferior portion, clearly within reach of the fore-finger, which, indeed, on making an examination, must evidently bear upon it immediately on passing the anus." Mr. Stanley under the head, "2nd. specimen.—From a male," is less accurate in his account of this preparation, but Dr. Ryan describes it very satisfactorily. "*One of these*" (specimens), says the latter, "*shows a considerable narrowing of the rectum below the sigmoid flexure: through the whole extent of the intestine, the parietes are better than three-quarters of an inch in thickness, and the canal is so reduced as scarcely to admit the passage of the little-finger through some parts of it.*" All these descriptions however are distinguished by one remarkable feature, namely, that not one of them contains even the slightest allusion to the existence of distinct, thickened, and shelf-like projections from the sides into the cavity of the rectum. Lastly, it is only necessary to bear this fact in mind, and to note those passages in the above statements which I have marked in italics, in order to see and be convinced, that Mr. Cooper is perfectly justified in considering this preparation to be a specimen of one of the three kinds of organic stricture of the rectum, which I have described in a note at p. 35 of my work, and to which I have given the name of "long stricture."

So much for the material difference which Mr. Salmon supposes, most gratuitously, to have existed between Dr. Blundell and Mr. Cooper. I have still, however, to advert to a third preparation, not adverted to by the former gentleman, but accurately described by Mr. Stanley, and briefly noticed by Dr. Ryan. It is thus described by Mr. Stanley:—"3. Specimen from a male.—The lower part of the rectum, to the extent of three inches and a half from the anus, is uniformly and considerably contracted. The mucous membrane is removed by ulceration from the whole of the contracted part of the intestine. Several fistulous passages extend from the ulcerated part of the bowel to the margin of the anus." Dr. Ryan notices it by saying, "In a third specimen there is a projecting irregular stricture, about an inch and a half in length." But I am either grossly deceived, or Mr. Stanley's description shows, that this preparation was one of fistula in ano, not of organic stricture. Viewing it in this light, it is easy to see the cause of the contraction observed at the lower part of the rectum.

In this analysis I have proceeded upon facts, not opinions, and the results are as highly favourable to my views, as they are unfavourable to Mr. Salmon's. But why has that gentleman confined himself to such evidence, as the comparatively few

preparations in his own possession are capable of affording, when I have offered him so wide a field as the great anatomical museums of London, for supporting his opinions? Such a mode of testing the correctness or incorrectness of my views on the subject, would have been perfectly conclusive; and I repeat, that I only wait to receive satisfactory proof of their unsoundness, in order publicly to renounce these views. Truth alone is my object, and I entertain no fear whatever of losing professional character, by abjuring an error entertained upon such seemingly strong and natural grounds.

I have the honour to be, Sir,
Your very obedient humble servant,

JAMES O'BEIRNE, M.D., &c.

North-Cumberland Street, Dublin,
Jan. 18th, 1834.

THE LANCET.

London, Saturday, February 1, 1834.

In the investigation and discussion of every great question, numberless important facts would be concealed from view, if self-interest, prejudice, and ignorance, did not come into actual collision with the opinions of men who are always devoted to the cause of truth.

It is amusing and gratifying to perceive the course which the corruptionists have selected in the contest for medical reform. Their antics delight us, because they are eccentric, and portend a full exposure of the peculiar attributes of those disputants; and their conduct affords us gratification, because it tends to a fuller development and comprehension of the important principles which ought to be scrutinized and understood by every individual who wields a pen, or hazards an opinion, with regard to a regenerated system of medical polity. It is the proud prerogative of truth to court inquiry, while falsehood ever shuns investigation.

The whole tendency, therefore, of the arguments of the corruptionists, points to a tame acquiescence in the existing "orders" and arrangements of the profession,—allowing, at most, that the disarrangements of the existing system should be subjected to the most gentle touch of the reformer's hand.

Accordingly, we hear of little else than the "impossibility" of renovating the scheme of medical government by bringing a new general principle into operation; or, indeed, when the corruptionists *do* condescend to admit, for one moment, that the thing is *possible*,—an attempt is made to scare us from effecting the desired change, by referring to the "greater miseries" which would be consequent on the adoption of the new system, than are to be occasioned by the old one.

We are sorry to be compelled to inform our opponents, that there is neither novelty nor ingenuity in their devices or their arguments. Their trick is one of ancient date, and their "reasonings" exhibit the coarsest machinery of legerdemain. Arguments advanced for the benefit of general practitioners in 1834, convey not the most agreeable sensations to the ear, when it is remembered that they emanate from parties who have not only embraced every opportunity, but have manufactured occasions, for abusing and treating with contumely that identical class of practitioners, during the seven years last past. Silence at this period would best become those adventurers in physic and in journalism who have thus misconducted themselves, who have thus, repeatedly, in the most wanton manner, stigmatized general practitioners in medicine, as the ordinary, the habitual, medical plunderers of the public. The system pursued by the physician in practice, has been extolled and represented as one of honour and purity, while that of the surgeon has been spoken of as mean, trading, and fraudulent,—invariably reflecting dishonour on the profession, and tending to the pecuniary and

bodily injury of the patient! Yet observe, the very writers who thus stigmatize general practitioners,—because those practitioners are *not* physicians, — are striving, by every means in their power, to prevent them from becoming "physicians," — to withhold from them the title of "Doctor." This one fact is a key which is fully adequate to unlock all those disagreeable secrets which the corruptionists are so anxious to retain in their own keeping. We marvel that they have the folly so to commit themselves! A schoolboy, a child, has sufficient penetration to unveil the mockery of their proceedings. The dread entertained by the BATS and their hired organs, arises from the supposition that *any* change would weaken the machine of monopoly, and, therefore, would prove conducive to the interests of surgeons, in general practice. Yet the short-sighted hypocrites profess to deprecate any extensive changes in medical law, on the plea,—generous souls! —"that an alteration might be productive "of additional disadvantages to the im-"mense majority of British medical prac-"titioners." Such is the lachrymose prophecy of one week, while the next furnishes us with a long string of ill-assorted statements, to show the "impossibility" of harmonizing the discordant medical interests of the United Kingdom.

In executing their disagreeable and compulsory toils, the corruptionists and the empty-headed pretenders to a knowledge of sound medical government, contrive to interweave, in the most amusing manner, the *perfectly distinct* questions of *medical practice* and *medical education*,—in the same manner confusing, with unenviable adroitness, the principles and details of professional and academic discipline.

Repeatedly have we urged the impropriety of branching off into questions of minute management, until we are agreed upon the great principle which the whole of our regulations are constructed to enforce. But it would appear that many individuals are capable of clearly understanding the structure and movements of a single wheel of a machine, the entire capabilities and uses of which machine are not embraced within the scope of their mental vision. For example; a man may readily see the convenience and utility of voting by ballot, with regard to his own individual security, but he may, at the same time, be incapable of estimating the value of that mode of voting, with reference to the affairs of the nation generally. In a word, his view of the utility of secret voting is limited to the sphere of his own action. Lead him beyond that point, and he is confused and lost, and either professes an utter indifference to the *system*, or, from not understanding it, and fearing therefore that its adoption might operate against his own interests, opposes its recognition by every means at his command. It is to be feared that the corruptionists and their hired agents approach, in some measure, this unfortunate state of selfishness and mental imbecility. On any other supposition, it is not easy to understand their conduct or appreciate their motives. GENERAL PRACTITIONERS are condemned *en masse* by the paid agents of corruption, by the M.D.'s who wield a pen in the cause of medical reform. The calumnies written and circulated against those surgeons who supply their patients with medicines, are unceasing; and yet the vituperators would withhold from the injured parties the only means by which they could pursue, according to their own statements, an honourable course of professional policy. We now altogether drop the question of medical education; we relinquish, at present, every consideration connected with the acquirement of medical knowledge by students; we examine only the principles which should regulate medical practice,

and thus direct our attention to the legal rights of medical practitioners. This is a simple question, and should be discussed singly.

In the first place, then, we ask, why men of equal attainments, and of equal utility to the community, should be designated by a variety of incompatible titles. Is the respectability of the profession promoted,—is the importance of that profession more effectually impressed upon the public mind, by awarding to a few *hundreds* of that body a title which is withheld from the *thousands* of its members? That title has always been bestowed without reference to superiority of attainments, and, thus, by awarding to the lowest in the scale of information the highest degree of reputation in medical learning, it has happened, most unfortunately, that the qualifications of the immense majority of the profession have been tested by a standard at once false and insulting.

Assuming, however, the position taken by the enemies of medical reform, and admitting, with them (of course for the sake of argument only), that the system of practice adopted by physicians is a protection both to the honour of the practitioner, and the *pecuniary* interests of the patient, why, we once more ask, is the bulk of the profession,—why, we ask, is the community,—why, in a word, are these two parties denied the advantages which the enemies of reform themselves insist should arise from the adoption, by all practitioners in medicine who might be rendered capable of enjoying the higher privileges of the profession, of the system pursued by physicians? As the practice of the physician is fraught with so many advantages, it seems extraordinary that such pains should be taken to prevent all competent surgeons and apothecaries from participating in a system which is so conducive to the integrity of the profession and the welfare of the public.

Now, it is quite evident that this view of the subject completely exposes the cloven foot of the enemies of a TRIPARTITE FACULTY OF MEDICINE. If those individuals were really desirous of relieving the majority of medical practitioners from the inconveniences and annoyances which render the discharge of their professional duties irksome, and often in the highest degree painful, they would willingly pass over all minor points, and zealously assist in placing their brethren in a position which would render professional trickery a less tempting adjunct of medical practice, and thus protect the public from the infliction of the, so by them styled, "scandalous draught-and-powder-system." Had we been natives of Utopia, we might have listened with a moderate share of calmness to the allegations which have been made that the practice of physicians is of a purer character than that of surgeons who furnish their patients with medicines. But when we perceive, week after week, that attempts have been made, and are still making, to represent that general practitioners in medicine are men of "insufficient education,"—that they are members of the "subordinate" ranks of the profession,—that they are incapable of undertaking the treatment of "important cases,"—and, worse than all, that it is their daily practice to plunder the sick by furnishing, first of all, an unnecessary supply of medicines, and then making extortionate charges for the articles so supplied,—our indignation, on reading such atrocious calumnies, is only controlled by our desire to expose and punish the calumniators.

Yet what are the two positions now severally occupied by the corruptionists and the surgeons in general practice?

The first, taking their stand in the buildings of the old corporations, insist on the inviolability of charters, and protest that the annihilation of the distinctive titles of

medical practitioners would prove dangerous, nay, ruinous to the profession. At the same time this very party stigmatizes the practice of nine-tenths of the profession, as being dishonourable to surgeons and apothecaries, and in the highest degree injurious to the public.

The second insist on an alteration of the medical system, and demand that the change should be founded upon the strictest principles of justice, thus ensuring to all legally-qualified practitioners an equality of professional rights and privileges in the three national divisions of this monarchy.

If, then, the majority of the profession are enabled, according to the statements of their calumniators, to cheat the public out of some thousands of pounds annually, surgeons and apothecaries—in supporting the proposition for a TRIPARTITE FACULTY of MEDICINE, cast back the imputation into the teeth of their unprincipled slanderers, and furnish the best possible proof of their disinterested and honourable feelings, by demanding a legislative enactment, which would deprive any unprincipled practitioner of even the *opportunity* of rendering his patient the victim of professional extortion.

We strongly advise a certain class of physicians to exercise a little more caution in their references to the professional labours of general practitioners. There is one numskull who is for ever vociferating "Ethics!" "Ethics!" and attempting to stigmatize surgeons who supply their patients with medicines, by calling them "traders!" Why,—does not this blockhead know that the strength, the prosperity, the naval and military glory, the literary and scientific reputation, of the country are all indebted for their existence to trade, to barter, to that exchange of things between man which gives health and stability to the body, and vigour to the mind? If this irredeemable dolt will but take the trouble to appeal to his grandmother, he may learn that he himself might have starved in the cradle, if his father had not carried on a flourishing trade in matches.

WE certainly should not have inserted a letter of the extraordinary length of that of Dr. O'BEIRNE, inserted at page 712 of this week's LANCET, if it had not exhibited to the profession, facts and arguments which appear to us to be of very great practical importance. Should we be called upon to insert any other communications on this subject, we must necessarily exclude them from our columns, if they would occupy more than three, or at most four, pages of our journal.

Whence the necessity of so much writing on a subject which admits of demonstration? Dr. O'BEIRNE has appealed to facts; Mr. SALMON has appealed to facts; but we rather believe that these gentlemen are not exactly agreed on the particular facts respecting which the appeals are made. Hence arise the discrepancies of statements and opinions. Without entering upon the merits of the question, we are strongly inclined to believe that Dr. BLUNDELL and Mr. STANLEY did not understand Dr. O'BEIRNE's peculiar and original view of "organic or permanent stricture of the rectum," and that Mr. BRANSBY COOPER, although his statement was not drawn up in the most lucid manner, entertained a very clear knowledge of the question at issue.

THE CULPRIT MACLEOD.

INSTEAD of appointing "arbitrators," and thus at once testing the accuracy of our statements by an inquiry, conducted *viva voce*, Mr. TUCKER has this week favoured us with a *third* letter, which we here insert.

To the Editor of THE LANCET.

SIR,—I must protest against the inference you are pleased to draw from my second letter, and I beg once more to repeat in express terms, that I never was authorized, directly or indirectly, by Dr. Macleod, to make any communication whatever to you. Moreover, I never told Dr. Macleod one word of what I had said of him to you. Let justice then be done on all sides. As far as *I* am concerned, I am most willing to submit to any investigation of this case; and if I can then be proved guilty of wilful misrepresentation, I must endeavour to bear with patience the odium I should merit.

I should have been wanting in that duty which one man owes to another, had I allowed Dr. Macleod to have remained unjustified; with this impression I wrote my first letter to you, and a similar one to the editor of the *Medical Gazette*. Finding I had not in that letter (written on the spur of the moment) done you equal justice, I wrote a second. This also, which was well meant towards both parties, has been misunderstood on your part, and taxed by the editor of the *Gazette* with a want of elegance. Probably, had either of you spent three successive and sleepless nights from home, attending those arduous duties which devolve on the accoucheur, great and undoubted as your talents are, you might have been found deficient, when called on to write upon so delicate an occasion. I trust, Sir, this may put an end to a very unpleasant subject; for being neither Whig, Radical, nor Tory, I am desirous to have done with medical politics. I remain, Sir,

Your obedient servant,

J. H. TUCKER.

17, Howland Street, Jan. 29th, 1834.

As for MACLEOD, he is now nailed down fast at our feet, and we defy him to run off into any hole or corner, and thus escape from the pummelling and flagellation which are due to this exquisitely "moral" worthy. It is quite useless for Mr. TUCKER to "protest" against our "inference," because his own admission of the accuracy of our report, will operate far more forcibly on the minds of impartial observers, than any meaning which, as interested parties, we might attribute to his words. What said Mr. TUCKER in his second letter?—"When Dr. MACLEOD told me that "he was *not then the Editor of the Gazette*, "and that many of the articles for which "*you had attacked him were not written by* "*him*, I said, *I shall tell* Mr. WAKLEY *this*. "He *laughed*, and we parted." Yet Mr. TUCKER now states, that he "never was "authorised, *directly* or *indirectly*, by Dr. "MACLEOD, to make any communication "whatever" to us!

Having thus, *on the authority of* Mr. TUCKER *himself*, shown, most indisputably, that MACLEOD *cheerfully acquiesced* in the proposal, made by Mr. TUCKER, to communicate the disclaimer of MACLEOD, that he was the Editor of the "Gazette,"—that he was the writer of many of the articles contained in that work directed against the private character of Mr. WAKLEY,—we have merely to refer (as Mr. TUCKER has omitted to appoint arbitrators) to the gentleman who was present at the interview in Bedford Square, for *his* account of the character in which Mr. TUCKER appeared on that occasion, and of the authority under which Mr. TUCKER professed to act.

To the Editor of THE LANCET.

SIR,—I have read Mr. Tucker's letters, Nos. 1, 2, and 3, and, on your application, willingly afford you my testimony, in writing, of the accuracy of the statements made in THE LANCET of Jan. 18th, page 643, relative to a communication which was made to you by Mr. Tucker in the autumn of 1830. The interview occurred in the library of your house in Bedford Square while I was present, and I declare that your report of it is correct to the letter. After the conversation, your presence having been required in another room, Mr. Tucker remained in the library with me for some time, when the communication he had made to you became the subject of a second conversation, in the course of which Mr. Tucker distinctly told me, that he had Dr. Macleod's authority for stating to you that he, Dr. Macleod, had ceased to be the editor of the *Gazette*, and that he, Mr. Tucker, had brought that information to you, at the express wish of Dr. Macleod himself.

I am at a loss to account for Mr. Tucker's want of memory, as I am ready at any moment to verify what I have here written, and the accuracy of your pub-

lished statement respecting the interview in question, in any manner Mr. Tucker may require. I am, Sir,

Your obedient servant,

GEORGE I. MILLS.

Chambers, 8, Clement's Inn,
Jan. 30th, 1834.

Here, then, the evidence is complete. Mr. TUCKER has testified to the statements of MACLEOD, and Mr. MILLS has testified to the statements of Mr. TUCKER.

When Mr. TUCKER speaks of "the duty which one man owes to another," we ask him whether it was consistent with a conscientious discharge of his duty to Mr. WAKLEY to refrain from forwarding his second letter to Messrs. LONGMAN for *printation* in their journal? This was a sad error of judgment, if not a serious breach of duty. MACLEOD of course is too "moral" to print such a statement against himself, but, fortunately, the non-appearance of the document in his pages cannot prove injurious to us, inasmuch as it accompanied the first communication,—*the all-in-all denial note*—in the columns of THE LANCET of last week. Poor MACLEOD is thus unfortunately circumstanced in dropping his falsehoods in and around the caves of the BATS,—that the exposure of those falsehoods in THE LANCET, is universal.

INTERCEPTED LETTER.

(ADVICE TO A YOUNG LONDON PHYSICIAN.—LETTER 3.)

"MY VERY DEAR YOUNG FRIEND,—The flattering reception you gave my former letters prompts me to enter further on the subject, and to continue to state to you freely and candidly every circumstance which a long experience has taught me to be proper and necessary to secure your success as a *London* physician.

"It may not seem to become me to tell you, yet I do not hesitate to say, that no man has been a more accurate observer of the world, or has profited more by it, than myself; and few men can give a better account of themselves, or more useful hints turned to the account of others.

"You ought never to lose an opportunity of claiming those advantages which the *Fellowship* affords. Always keep a sharp look-out for offices of emolument in the College which may fall vacant. I have always, without exception, appointed Fellows to every situation which yielded *emolument*. I have already mentioned to you that we have in our time had many hospitals in our hands, as snug and as close as Old Sarum. We are not, therefore, without good things to bestow. Then, again, the Commissionerships of Lunacy are very good things. We had the Vaccine Board, also, entirely at our disposal, but there our wings have been a little bit clipped lately, which is unlucky, for besides giving Dr. Turner and several others small convenient salaries, there was by no means a despicable sum coming in to the President, which had I been disposed to give up, would have been a nice gift to somebody else who wanted an office and a salary with no labour, and who, by the good offices he had done the College, in the way of speaking or writing for it, deserved such a reward. There was also the original Cholera Board—our own board—every thing our own way, which was so organised as to secure tolerable salaries to ourselves. I had a part of that, and had the bestowal on other Fellows of those parts which I did not want or could not fill. Unfortunately the Board was blown up by the destructive journal, and the Government instituted a new Board, on what one of our friends in his peculiar spicy language called, their "d—d economical principles," and in place of peaceful gentlemanly Fellows, appointed a parcel of military officers, a tenth part of whose bustle and movement would have rudely shaken the entire dust out of every wig in the College. The mention of these things, now they are gone, may seem out of place, but it shows you what we occasionally have to bestow on meritorious aspirants.—Then, also, there are several chairs which we are able to get for Fellows, besides the professorships at Oxford and Cambridge. I secured those of King's College for my own relatives and dependants, and though they yield little emolument, and perhaps are not likely to do better in our days—yet they give a man *a name!*

"Again, though I don't expect ever to be able to do such things again, yet it was entirely through me, the President of the College, that Dr. Badham was sent to Glasgow, where I believe he is universally esteemed, and is considered as one of the most profound, learned, and eloquent lecturers of civilized Europe. At least, he writes me word that such is the fact. Since that appointment, too, he has been

successful in getting engagements to write for some of the London monthly magazines. I have also succeeded in securing the patronage of the court, which the Fellows of our College have always considered as a strong and imposing position for a physician to occupy. They were never able to exclude licentiate physicians until I managed to get our most gracious sovereign, King William, to permit me to nominate the whole of his medical staff. I took care at once to turn off all licensed physicians and radical surgeons, though in doing this every kind of odium was heaped upon me by the public press, which I did not heed, for the sweets of the arrangement outweighed the bitters. I was even accused of *illiberality* for securing, as was alleged, the most lucrative situations to myself! But let us leave this part of our subject.

"In conducting professional consultations you must always observe the strictest etiquette, and keep licentiates, surgeons, and apothecaries, each in their proper places. The term 'consultation' is indeed absurd; that of 'dictation' would be more appropriate, you of course being always the *dictator*. Dictate to the licensed physician the prescription. Let the surgeon *propose* for your *approbation* some external nostrum, and give the prescription to the apothecary to have it properly compounded. But never permit a subordinate to change your prescription, whatever change may take place in the symptoms. In stating the opinion of 'the consultation' to the patient, talk in the plural number, but if you have the opportunity of a *side wind*, give him an impression that *you* have got some *new* key to his complaint, and that the others have been working in the dark. When you have any trouble with either of the other practitioners, in place of keeping your next appointment, call alone. Many eminent men find this plan answer their purpose well. If at any time you should have reason to suspect that some physician is getting the ear of a high patient, who ought from his rank to belong peculiarly to *us*, and you are called in to consult, be there half an hour beforehand, when, by devoting so much time to the female portion of the establishment, you may create an impression in the family of no little value to your own reputation. Consult on some of these points with your father's old acquaintance, Sir William Knighton. No man better understood London practice, or stored more profitable and rational maxims for making one visit of importance to another.

"However trifling the case may be, or however unnecessary the visit, never fail to allot to your patient a guinea's worth of time. Always write a new prescription at each visit, and write it upon a large piece of paper. Write it slowly, and distinctly, and let it all be in *Latin*, whether good, bad, or indifferent.

"Though it is of great importance to make patients narrate the most frivolous particulars of their case, I do not approve of writing out cases. Old Sir Gilbert Blane used to keep a journal of every case he attended, and sometimes it had a good effect to read to a lady an account of her case and all her complaints for the last twenty years. It is very well to impress on those families which you regularly attend, the superiority you have in knowing intimately their *constitutions*; but when you are consulted by a stranger, then talk of the advantages of a 'fresh eye' and an 'unprejudiced mind.'

"You cannot appear to be too accurate in feeling the pulse, counting it over and over with a watch several times in each arm, and when you have occasion to order a purgative, desire each evacuation to be kept in a separate vessel, which, besides showing attention, ensures the necessity of a visit on the following day. You need not take much pains in looking at the contents of the vessels. The *effect* of the directions is sufficient.

"Be also most particular in fully considering and warmly entering upon the subject of eating and drinking, for however ill patients may be, the 'animal' is always uppermost with them. Assist them in various devices to gratify the appetite, and daily suggest new dainties, purging away the ill effects afterwards. This creates at once a liking for you, and a continued demand for your prescriptions. Of all things beware of getting the character of a *starving* Doctor; it would ruin your practice. Allow your patients, on the contrary, to eat and drink whatever they like.

"I am at a loss what advice to give you about the stethoscope, as I know nothing about it myself. It is a farce to talk of its being of any use in the practice of physic, but still you had better learn how to employ it, as all patients like to have their machine, however rickety, accurately examined, or at any rate *apparently* so. But you must be very careful how you commit to paper any opinion professedly drawn from an auscultation with that foreign instrument. There have been some confounded blunders made with it, particularly by the physicians of St. George's. But the grossest of all blunders that ever was committed, was by one of our licentiates—Dr., now Sir, Charles Barry, wherein he committed himself in the most injudicious manner. All the par-

ticulars you will see fully detailed in an old number of the inconvenient journal. It was a case of aneurysm of the innominata, —I think they call it—which was operated on as such by that eminent surgeon, Mr. Wardrop, and, somehow or other, poor Barry, who, at that time, was always bragging of his stethoscopic dexterity, most unfortunately for himself pronounced it to be a disease of the heart. He foolishly committed this opinion to paper, and was thoughtless enough to send a copy of it to the editor of the journal I have mentioned, to be published *after the woman's death*! The case, as every one was prepared to hear, turned out to be an aneurysm of the innominata, and the editor accorded with Barry's solicitations, and published to the world his fearful blunders.

"Regarding *fees*, the most important of all subjects for your consideration, we are often difficultly situated. If your expectations and charges are too extravagant, you lose your patients. At the same time you have every right not only to keep up the dignity of the profession by charging well, but to get as much money as you honestly can. The London physicians are now a days, when the world is revolutionizing, so hard pressed by the surgeons and the 'subordinates' (I got this happy word in conversation with my very dear friend Brodie; it is better than 'draff') that I am at a loss how to advise you to proceed. Surgeons attend any patients who choose to employ them. Sir Astley was very angry at a young friend of mine for recommending a patient of his, who had an affection of the liver, not to consult the baronet, because, forsooth, it was a 'physician's case!' Patients, however, will do as they please, and the one I mention went to Sir Astley, whose opinion he was determined to have, and a most excellent opinion he got, which led him to tell the baronet that he had been advised not to consult a surgeon but a physician. 'Make my compliments to your friend,' was the Baronet's reply, 'and tell him that I am always happy to give advice *in all cases when I can be useful*.' Another pure surgeon, who shall be nameless, had the impertinence to say, when he was asked the distinction between surgical and medical cases, 'that he considered all patients within his province, except those that required a great deal of attendance and had no money.'

"At present I have not time for a word more. I have to-day to complete my paper to be read at the first meeting of our College for the new season, and after that to visit the Premier, the Secretary of State, the Lord Chief Justice, the Bishop of London, and a few more high characters, to try to persuade them, especially the two first, to honour me with their presence on that interesting occasion. I always contrive to bring the ministers who are in office for the time being to those delightful and dignified assemblies, though I hope they will not, like my Lord Brougham, this time twelvemonth, fall asleep, and actually nod beside me. Farewell, and believe me, ever your friend,

"——— ———

"Jan. 25th, 1834."

ROYAL COLLEGE OF PHYSICIANS.

Monday, January 27, 1834.

FIRST PERFORMANCE FOR THE YEAR.

New dramatic seasons are always prefaced by the issue of advertisements from our theatres, calling upon the "ladies and gentlemen of the establishment" to assemble on the various stages at such an hour, to prepare the arena of gaiety for the approaching campaign. At the Theatre-Royal, Pall Mall East, the actors are summoned in a different and more private manner, the public advertisement only extending to the house-maids, tea-makers, and door-keepers. In obedience to the call for the season of 1834, these most important portions of "the establishment" assembled on Monday morning last within the walls of the building, and under the direction of the admirable and faithful stage-manager Macmichael, swept and dusted the apartments carefully, laid the fires, prepared the lights, cut 2000 slices of bread-and-butter, prepared 500 muffins, and brewed 30 gallons of Mocha coffee, to the great honour, glory, and hospitality of themselves, the President, and the rest of the dramatic company. Unusual diligence was employed on this occasion, as the great President had intimated, that in consequence of the prevalence of some reports that the establishment was in its wane, he intended that night to produce a scene equal to anything in *Gustavus*, then performing at one of the other houses.

The rumour of unusual brilliancy spread beyond the walls, and drew an immense audience. Spectacles—it is the lament of the day—fill the houses, while the "legitimate drama" is played to empty seats. Pit, boxes, and even the gallery, were crowded. A recitation was announced from the President, in which a dissertation on medical politics, and a discourse on "the beauty of things as they are" was expected to be embodied. At nine Sir

HENRY entered. In his train were many magnates of church and state, and a group of actors from another theatre of twin interests. Amongst the former were, Earl GREY, the Marquis of LANSDOWNE, Lord MELBOURNE, Lord ALTHORP, Lord DUNCANNON, the Lord CHIEF JUSTICE, the Lords Bishops of LONDON and LLANDAFF, Lord JEREMY DIDDLER, Sir ASTLEY COOPER, the PRESIDENT of the COLLEGE of SURGEONS, and Baron VAUGHAN. With the Ministers and Bishops ranged on each side, Sir HENRY seated himself beneath the bust of "Pater patriæ," and in a sonorous tone pronounced his text to be "The Education and Duties of a Physician." A report of the discourse would be a superfluous detail to the readers of THE LANCET. Suffice it to say, that it was a highly characteristic effervescence of the peculiar genius of this Doctor-courtier. Avoiding an iteration of his advice in our pages, let us observe that in other respects the *verba lactis* flowed in an unbroken stream, wafting here a compliment to the church,—there an encomium on the bench,—now a tear to a fallen Minister,—then a *bon-bon* to his triumphant successor, — all luxuriantly interwoven with a lace-work of erudite phrases and scraps of classic lore. But the anticipated effusions of medical diplomacy where were *they?* Surely the fertile topic of "education" would afford occasion for some adroit allusion directed to the ear of the mild Premier, and the influential Secretary of State! Yet alluring though the time, it was "in the bond" that *silence should be kept by the monopolist* on the interesting topic, and no pledge thereto was broken. The orator wandered in no directions, east or west, but such as led him to his favourite regions, Oxford and Cambridge, whose "wholesome air" he declared to be peculiarly effective in purifying the blood of young physicians, and of paramount advantage to the lungs during the training of embryo "Fellows" for the efficient practice of medicine,—the study of disease being left for after consideration in London, or some foreign school—after the manner of "the great BAKER (Sir GEORGE) and other eminent physicians." The sciences of the mathematics and logic, with anatomy, chemistry, and botany, elicited some remarks; but to the classics were assigned the rich reward—they were the prolific fountain—of all science! We can afford space to notice but one other feature of the oration, and for this was reserved the greatest share of brilliancy in style and tone. "Manners make the man," said the courtier, and from this axiom he deduced "of fine precepts a score." The profitable equilibrium between the "suaviter in modo," and the "fortiter in re," was cunningly shown forth. The chapter "Mores" was, in fact, in *point d'appui* in the discourse, whence was displayed the "whole duty of a Fellow," to the wondering ears of the audience which had there attended to learn manners from the supple President of the Royal College of Physicians! The distribution of public advice on such a subject surprised some of his Majesty's Ministers not a little,—such of them, that is, as slept not, for one noble lord took refuge in a nap. My Lord MELBOURNE had to lay violent efforts on that sarcastic levator anguli oris which is so active among the muscles of his face, lest it should be o'erstrained at the measure of impertinence with which the crowd of gentlemen around were treated in this trumpery divergence upon good breeding.

The conclusion of the entertainment afforded nothing beyond the usual accidents attending the consumption of tea and coffee in well-crammed saloons.

LONDON MEDICAL SOCIETY.

Monday, January 27th, 1834.

Mr. KINGDON, President.

DISEASED GRAIN.

WE have room for only the following brief notice of a part of the proceedings of the above date.

Dr. WHITING said that he observed in the room a gentleman (Dr. Tytler) who had, during a debate which occupied four successive nights, communicated to the Society much valuable information on the subject of diseased grain, and he should therefore seize that occasion to state, that he had recently been called to a child that was affected with inflammation and gangrene of the toe, which subsequently extended up the foot and leg, and at length terminated in death,—events affording him reason to believe that the disease originated in the use of diseased grain as food, and tending to corroborate some of the important facts and opinions submitted to the profession by Dr. Tytler.

Mr. HEADLAND also took the opportunity of relating a series of extraordinary facts which had recently occurred in a public institution, strongly corroborative of the statements made by Dr. Tytler; but as the occurrences referred to are at this moment *sub judice*, we do not feel at liberty at present to advert more particularly to them.

The PRESIDENT observed that there could be no doubt that the discussions in the Society in October last, had attracted

the attention of the profession and the public in a strong manner, and that vitiated grain had obtained such a character therefrom, as had produced a high and previously unexcited degree of caution with respect to flour and grain. He thought he spoke the sentiments of the Society when he observed that the thanks of the Society had not been mis-bestowed when they were given to Dr. Tytler at the termination of the discussion on the rice and cholera question. He felt indeed that every inhabitant of the British islands was indebted to Dr. Tytler for the perseverance he had displayed in bringing his very interesting facts before the Society.

Dr. TYTLER offered his acknowledgments for this compliment, and made some extensive remarks on the rice question, comprising a variety of circumstances connected with the subject, which had been disclosed during his absence from London on a visit to the metropolis of Scotland. The meeting was then adjourned until next Monday, with the understanding that Dr. Tytler would then attend, and introduce the topic in a more formal manner.

MEETING AT LIVERPOOL.

MEDICAL REFORM.

AN aggregate meeting of the members of the medical profession residing in Liverpool, called in pursuance of the resolution entered into by the Liverpool Medical Society, was held last week at the Medical Library in that town, Dr. RUTTER in the chair. A hundred medical gentlemen were present. A resolution to the following effect was then moved by Dr. H. LANE, and seconded by Mr. G. ROGERSON, and ultimately carried, with one dissentient only:—"That the partial and restrictive distribution of privileges legalized by the different charters at present possessed by the various colleges throughout Great Britain—the numerous sources of medical licenses—the discordant, and in some instances, imperfect systems of professional education allowed by universities—the extremely defective preliminary classical education of great numbers of young men now introduced into the profession—the insufficient proof of competency, upon which individuals may gain legal permission to practise the art and science of medicine—the exclusion of persons (already recognised by British medical incorporations as duly qualified to exercise the healing art) from practising in certain districts—the improper interference of the Apothecaries Act with the right of well-educated physicians and surgeons everywhere, but particularly in rural districts, to prepare and compound medicines for their own patients—the non-protection afforded to legitimate practitioners against collision with uneducated empyrics—the tacit permission given to quacks and all unqualified persons to exercise the functions of educated members of the profession—and the fact so universally prevalent, of druggists prescribing medicines, to the danger of his Majesty's subjects, as well as to the disadvantage and prejudice of those legally authorised, are circumstances which, in the opinion of this meeting, evidence the imperfections of the laws of the medical profession, and prove the necessity of legislative interference."

Proposed by Dr. MACINTYRE, and seconded by Mr. VAN OVEN:—"That a committee be appointed to draw up a petition embodying the grievances represented in the resolution just passed, to be forwarded to both Houses of Parliament praying for the appointment of a *Parliamentary Committee of Inquiry* to ascertain the actual defects of medical legislation with a view to their removal."

A committee of seven was then appointed to draw up a petition embodying the spirit of the resolution. The committee is to correspond with the Medical Associations of London, and to adopt such other measures as it may deem requisite for the promotion of medical reform. The following gentlemen were named on the committee:—Dr. Rutter, Mr. George Rogerson, Dr. Jeffrey, Mr. Christian, Dr. Lane, Dr. Macintyre, and Mr. Van Oven.

After passing a vote of thanks to the Chairman, the meeting adjourned.

NOTICE.—It is particularly requested that ALL letters of business forwarded to the Office of THE LANCET,—whether they relate to the contents of the journal, or refer to transactions connected with the Commission Department,—be INVARIABLY addressed to THE EDITOR—Mr. Wakley. If this rule be not observed, it is impossible to guard against the impositions of anonymous scribblers who appear to have no other employment or pleasure than that of subjecting the conductors of journals to a heavy extortion in the shape of post-tax. If all letters of business transmitted to the office, be addressed to the Editor, and NO letters, unless so addressed, be received except the postage be paid, impositions will be effectually prevented.